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The Concept of Entropy

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THE purpose of forming the concept of entropy is to interpret—and notice that I do not say "explain," I say "interpret"—the fact that certain processes of Nature go of themselves in one direction, and cannot at all be made to go in the other. After such a spontaneous process has gone its predestined path, the world can never again be the same.

We do not have to go to a laboratory to find examples of these processes: they are only too familiar. A man proceeds from the state of being a baby to that of being an old man, and never in the opposite sense—except in *Faust*, and there it took the devil to restore the initial state. A house afire proceeds in the direction to ashes, but the ashes never turn back into the house. You can't unscramble eggs, as a famous financier is said to have remarked. The cup of coffee on the breakfast table beside the eggs, if violently stirred and then left to itself, will pass from the state of being a whirlpool to the state of motionless tranquillity. Never will the coffee pass of itself from the state of tranquillity to the state of being a whirlpool; if it ever should, the most untutored and the most impassive of breakfasters would spring from his chair and search for the cause or take to flight, depending on the extent of his curiosity and his courage. Gas in a tank will leak out through a hole, but it will never gather itself together out of the atmosphere and leak back into the tank to displace the air which replaced it.

Events such as these are interpreted by saying that there is something called "entropy" which has a natural tendency to increase and is forbidden by Nature ever to decrease. If the eggs were to unscramble themselves, the coffee to start whirling of itself or the gas to rush back from the atmosphere into the tank, the entropy would go up; but this is forbidden by Nature, and that settles the question. This is one of the looser ways of stating "the second law of thermodynamics."

Now of course the meaning of the foregoing paragraph amounts to precisely nothing unless it is possible to give a numerical measure—in other words, unless it is possible to *measure* this thing called entropy. It is a triumph of the science of thermodynamics to have taught how to measure entropy, or change of entropy rather, in certain simple cases. It is a triumph chiefly of chemistry, partly also of classical physics, to have found many cases in which entropy *can* be measured according to the precepts of thermodynamics. It is a triumph of modern physics to have found a model or picture of entropy. These three achievements are the topic of this article; but since each by itself is enough for many articles, the reader must not be surprised that the treatment of each is superficial; nor will he, I hope, be surprised to find that no such complicated cases as those of the aging man, the burning house or the scrambled eggs find any place in the discourse.

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The *way in which change of entropy is to be measured* is taught us by thermodynamics: and this is it.

Imagine a body, or a system, or an object—whatever you may choose to call it—at a definite temperature, which we shall denote by T. Suppose that into this body there flows from the outer world an amount of heat to be denoted by dQ, too small to warm up the body appreciably. To the inflow of heat into the body there corresponds, or by it there is entailed, a rise in the entropy of the body which we shall call dS. The relation between the rise of entropy and the inflow of heat is the following:

$$dS = dQ/T. \qquad (1)$$

A very simple relation indeed! but it requires a good deal of precise stipulation, or hedging-about, only a part of which I shall be able to give here.

The first stipulation is, that when the heat flows in nothing must occur of a tumultuous, an agitated or an explosive nature. If the body explodes, or if it caves in, or if it starts to thrash about, or any sudden chemical reaction happens, or anything of the sort which a catalyst must be invoked to promote—if any of the spontaneous or "one-way-street" processes occurs in the system during the inflow of the heat, the formula is almost no good. One is still permitted to say that the rise of entropy cannot be less than dQ/T, but this is of no use for measurement.

The foregoing is an attempt to put into dramatic words the thermodynamic statement that when the heat flows into the body, any and all of the changes incurred by the body must take place "in a reversible manner." *Reversible* is here the charmed word. It is taken for granted that chemists and physicists who work in this field are able to conduct a process reversibly, or at least that when the process is not proceeding reversibly they can recognize the fact.

(Suppose that when the heat flowed in, an irreversible process did inevitably occur in the system; or in other words, that we knew of two states of the system, but did not know of any way of proceeding from one to the other which did not involve the happening of something irreversible in the system itself—what then could we do to measure the change in entropy from the one to the other? The answer is: *nothing*, unless we may call upon theory to fulfil the defect of practice.)

The second stipulation is, that the temperature T must be measured in a very peculiar way. It is obvious that Eq. (1) will give different results according as one uses temperature Fahrenheit, or temperature centigrade, or temperature Réaumur. Actually all of these results would be wrong. There is only one which is correct, and that is obtained by using the so-called absolute, or Kelvin, scale of temperature.

In practice the absolute scale of temperature is found by asking the National Bureau of Standards, or a similar institute, for a table of conversion from centigrade to absolute. In principle it is the scale of temperature which makes the second law of thermodynamics agree with experiment. The second of these statements sounds about as frivolous as the first, but actually it is a serious one. The second law of thermodynamics challenges the test by experiment at so many points and in so many ways, that when one single scale of temperature can make it come out right in all of the tests, that in itself makes it a law of Nature. The Kelvin absolute scale uses the centigrade degree,[1] but is reckoned from a zero—the "absolute zero"—about 273.15 degrees below the centigrade zero. It can always be ascertained until the temperature gets so low that no substance remains gaseous and of appreciable density—an embarrassing situation which is reached at about 1°K.

Now let us consider two bodies, one hot and the other cold, and heat flowing from the hot body to the cold. One may imagine the cold body as quite enveloping the hot one, so that the heat which the latter loses has no place else to go. We shall find out what happens in respect of entropy, when a quantity of heat dQ too small to affect the temperatures of the two bodies passes from one to the other. These temperatures shall be denoted by T_h for the hot body and T_c for the cold. As nobody needs to be told, the temperature scale is so arranged by convention that T_h is greater than T_c: if it were otherwise there would be no change in the second law of thermodynamics, except the trivial one that what we call entropy would tend

[1] The size of the degree is of course immaterial; the centigrade degree is adopted for convenience.

to fall and be prohibited from rising, instead of vice versa.

During this process of the flow of heat dQ from hot body to cold, the entropy of the cold body ascends by dQ/T_c. This is what Eq. (1) says. Also, the entropy of the hot body declines by dQ/T_h. This again is what Eq. (1) says, though I did not point out the obvious implications of changing the sign of dQ. The entropy of the system goes up, because it is the sum of dQ/T_c and $-dQ/T_h$, and the latter fraction has the same numerator as the former but a bigger denominator.

The "system," *nota bene*, comprises all of the bodies involved in the process. The hot body and the cold body are just parts of the system. One of these suffers an actual decline of entropy, and this shows that the second law of thermodynamics is not to be applied to a part of a system by itself. The other part has experienced a rise in entropy large enough to offset, indeed more than large enough to offset, that decline. It is this compensation or overcompensation which makes the law valid for systems in general. I mention this because it is easy to get the false impression that entropy is prohibited from falling even for objects that are merely parts of a system.

Note next that this process of the flow of heat, which is attended by a rise in the entropy of the system, is a spontaneous and a one-way process. Heat flows of itself from hot to cold, cannot be made to flow from cold to hot. Here we have the archetype of all the "irreversible" phenomena, and the definition (1) of change-of-entropy has enabled the Second Law to cover it.[2]

Note lastly how this result forbids us from imagining entropy as a substance, or a fluid, or anything that is conserved. Entropy does not obey a law of conservation, except of course in special cases. There are quantities in Nature that do conform to a law of conservation. Momentum is one, angular momentum another, energy another, electric charge a fourth. Of any of these it may be said that if a certain amount of it appears at any place, the same amount must just have disappeared out of some other place—if it is received somewhere, it is taken away from somewhere else. Of entropy this cannot be said. Any of those four might be imagined as, say, a fluid wandering through the world, now here and now there, but ever constant in total amount. Entropy must not be so imagined. If in one part of a system there is a rise of entropy and in another part a fall, one should say it that way, and leave it at that; one should not say that entropy has flowed to the one part from the other. Entropy is an attribute of a body which stays *in situ*, rising or falling in amount, but not to be regarded as rising because it is coming from elsewhere nor as falling because it is going elsewhere.[3]

Our definition of change-of-entropy having been made and having been fortified with an example, I now proceed to make use of it in leading over to the next topic of this paper. Imagine a gas which, being initially at a temperature T and under a pressure p, is to be cooled to the absolute zero while the pressure remains the same. We are going to find the formula for the amount by which its entropy falls as it makes this descent in temperature.

A familiar gas, such as air or its constituent elements, would make this descent in five distinguishable stages: It would cool as a gas down to the boiling temperature appropriate to that particular value of p, liquefy, cool as a liquid down to the freezing temperature, freeze, cool as a solid down to the absolute zero. Account could be taken of all of these stages without notable difficulty, but for ease of exposition I am going to reduce them to two. The second and third of the five can be eliminated by choosing a value for the pressure p so low, that the gas passes directly from the gaseous to the solid phase without the liquid interlude. (If the gas be carbon dioxide, for instance, the necessary pressure need be no lower

[2] Sometimes a false analogy is drawn between the tendency of heat to flow from hot to cold, and the tendency of weights to fall from high to low. There is no true analogy, for weights can rise directly from low to high. You can roll a ball up a hill, but you cannot "roll" heat up a temperature gradient.

[3] I am rather embarrassed to realize how large a part of this paragraph is equally true of heat, which like entropy does not obey a law of conservation except in special cases. Yet we are all accustomed to thinking of heat as flowing from place to place, and in fact I have already used this language in the paper in speaking of the passage of heat from one body to another. Strictly, I ought to abstain as carefully from speaking of flow of heat as from speaking of flow of entropy. Yet experience shows that we do speak of flow of heat without incurring serious error, and the reason must be that the special cases in which heat is conserved are much more abundant and familiar than those in which entropy is conserved.

than atmospheric.) The first stage can be eliminated by the mere device of starting with the gas "just ready to freeze," to wit, at the sublimation temperature T_s, where gas and solid are in equilibrium for the particular value of p prevailing.

Now the descent from the gas at (T_s, p) to the solid at (0°K, p) has been subdivided into just two stages. The first stage is produced by slowly compressing the gas: The temperature remains at T_s and the pressure at p until the whole of the gas is converted into the solid. During this process the gas gives up its heat of sublimation, a definite and a measurable quantity which, for the mass of gas considered, I will denote by L. Its entropy thus falls by L/T_s in the first stage.

The second stage, during which the former gas cools down as a solid, must be subdivided into infinitesimal intervals of temperature, and the infinitesimal outflow of heat $\mathrm{d}Q$ during each such interval must be divided by the corresponding temperature. The total fall of entropy during this second stage is then the integral $\int \mathrm{d}Q/T$, which I rewrite at once as $\int (C_p/T)\mathrm{d}T$; for $\mathrm{d}Q/\mathrm{d}T$ is here C_p, the specific heat at constant pressure, since we are assuming that the pressure stays always the same.

The limits of integration are, of course, T_s and 0. Notice that our integral will be infinite unless C_p goes to zero as T goes to zero; formerly it was thought that this would not happen, and the first experiments which showed that it *does* happen were inspired by the hope of avoiding the consequences of the contrary assumption.

Now I condense the content of the last two paragraphs into the equation

$$S(T_s, p) - S_0 = L/T_s - \int_0^{T_s} (C_p/T)\mathrm{d}T, \quad (2)$$

in which the right-hand member is a measurable quantity; $S(T_s, p)$ stands for the initial entropy of the gas-ready-to-freeze at temperature T_s and pressure p, and S_0 stands for the entropy of the solid at the absolute zero. The reader will wonder why I have not written $S(0°, p)$ instead of S_0, since obviously it is the entropy of the solid at the absolute zero *and* at the particular pressure p that figures in Eq. (2). However, I do not need to introduce the symbol p, for *the entropy of any substance at the absolute zero is independent of pressure.* This is a principle (forming a part of "Nernst's law," or the "third law of thermodynamics") which was derived and tested more than 30 years ago, before the advent of most of the knowledge which appears in the remaining pages of this paper. The simplified symbol S_0 is therefore sufficient.

Evidently, if we knew by experiment either the value of $S(T_s, p)$ or the value of S_0, then the equation would enable us to find the other. But thermodynamics does not tell us how to find either the one or the other, nor does it ever tell us how to find the value of entropy for any particular situation. All it can tell us is how to ascertain the entropy-difference between two different situations; and this is what it has already told us in Eq. (1). We cannot go one step further without appealing to a theory. Fortunately there is a very powerful theory to which we may appeal.

This very powerful theory is the one known by the general name of "statistical mechanics," in the form specially known as "the new statistics" or "the quantum statistics." It is the theory associated with the names of Bose and Einstein, Fermi and Dirac. It is sustained by its success in explaining the Maxwell-Boltzmann distribution law for atoms and molecules of gases and for thermionic electrons, the Planck distribution law for blackbody radiation, the distribution of velocities of the electrons in metals, and other phenomena more recondite; and lastly it is sustained by the results here to be related. It is a strong and a sound theory, and here I shall feel free to take it for granted.

The importance of the new statistics for our present purpose arises from the fact that it supplies a complete theoretical formula for the entropy of a gas as a function of the pressure and the temperature thereof. Into this formula we have only to put T_s for the temperature and p for the pressure, and then we have the theoretical value of $S(T_s, p)$, which hereinafter I will call S_{th}. True, we have to know many things about the atom or molecule of the gas in order to fill out the formula; but these things are all known for most atoms and for many diatomic and even a few triatomic molecules. Therefore, Eq. (2) may be rewritten thus:

$$S_0 = S_{th} - \left[(L/T_s) + \int_0^{T_s} (C_p/T)\mathrm{d}T \right], \quad (3)$$

and now I come to the great fact of experience which is the second major topic of this paper:

For all monatomic gases and many diatomic gases, S_{th} is equal to the terms bracketed on its right, and accordingly S_0 is zero. The entropy of the solidified gas is zero at the absolute zero.

This is a generalization well substantiated but not so well known as it should be, because the experiments have mostly been regarded as chemical and have been published in the chemical literature, while the theory is regarded as physics. It is a generalization of sufficient scope to be designated as a rule, yet not as a rule without exceptions.

Now, the advance of science often travels the following route. Someone discovers, or thinks he discovers, a rule. Usually it has exceptions; so, the rule-giver tries to prove that these are merely spurious exceptions, due to faulty experiments or to mistaken conditions. If he succeeds in this, the rule is a law of Nature, and something to be proud of; but it becomes before long a sort of museum-piece of physics, and loses much of its interest for the creative workers. If, on the other hand, the exceptions pile up, then the rule itself may vanish as a transitory delusion. But if there are so many confirmations that the rule cannot be doubted, but also so many exceptions that the exceptions cannot be doubted—then the situation is really exciting! For then there is a rule of undoubted but limited meaning, and also a hope of finding a newer rule or rules to codify the exceptions. Such is the situation in this question of "zero-point entropy," as I will hereafter call it.

First, let us seek an interpretation of the rule. In all of the cited cases,[4] the substance at the absolute zero is a *crystal*. Furthermore, it is a crystal with the least possible thermal agitation of the atoms. Formerly people believed that at the absolute zero, the atoms stand still. If they stood still at the points of a perfect crystal lattice, then they would possess what anyone would call perfect order. If, as is now supposed, the atoms are wiggling around even at the absolute zero, then at any rate they possess the nearest approach which Nature permits to perfect order. Indeed I think that we ought to call it perfect order, for to a scientist there is probably no point in defining perfection as something which is unattainable in principle, though saints and poets may feel differently. *Zero entropy then corresponds to perfect order:* this is the interpretation of the rule.

Now we must not evade the exceptions. There are certain cases in which S_0, the zero-point entropy, is greater than zero. To attain a verbal harmony with the rule, we must say that in these cases a certain amount of "disorder" survives even at the absolute zero. This sounds all right for a beginning; but we are obliged to go deeper. We must inquire whether there *is* always in these cases something so clearly disorderly that anyone would naturally call it "disorder," and if the answer is *yes*, we shall next want to know whether there is any numerical measure of this "disorder" out of which we can extract a quantitative theory of the value of S_0. It will transpire that the answer to the first question is not always *yes*, so that the question has to be restated: in these cases where the zero-point entropy is greater than zero, is there always something peculiar about the substance on which we can impose the name "disorder," whether it looks disorderly or not?

To attack these questions, let us look to see where the exceptions are and where they are not. I have already said that elementary substances in a crystalline form are not exceptions: they obey the rule. This is true whatever type of lattice they affect. I for one have difficulty in visualizing any type of lattice but the cubic, and I should have been glad if the data had allowed me to disparage every other kind of lattice by calling it disorderly. This they will not allow. There are elements that can go down to the absolute zero in either of two crystalline forms, or "modifications" as the word is. Tin is a classic example, and sulphur another. *Both modifications show identical zero-point entropies.*

I take this occasion for showing how such a statement as the last one can be proved, even if one does not know the value of S_{th}. Consider the sum $(L/T_s)+\int(C_p/T)\mathrm{d}T$ which is deducted from S_{th} to get the value of S_0. If this sum has the same value for two modifications of a single element, then S_0 must have the same value for the two, and in view of the many confirmations of the

[4] There is one exception: helium, which at the absolute zero is *liquid* if the pressure is below 25 atm, a crystalline solid at higher pressures only. I will recur to this exception later.

rule, it is at least a strong presumption that this common value is zero. Or suppose that there is a reversible transition between the two modifications at some point (p, T_t) on the isobar p between the temperatures T_s and zero; and let L_t stand for the heat absorbed during this reversible transition. Then if the integral $\int_0^{T_t}(C_p/T)\mathrm{d}T$ for the one modification is equal to the sum of L_t/T_t and the corresponding integral for the other modification, the zero-point entropies for the two are the same, and it is again a strong presumption that their common value is zero.

But suppose there were a substance which could go down to the absolute zero *either* as a crystal *or* with its atoms not arranged on any regular lattice at all, but jumbled together like the bricks of a building struck by a block-buster? Glycerine is a fair example of such a substance. While my simile is probably too strong, glycerine can be chilled to temperatures very low indeed while its molecules remain in what anyone would call disorder; on the other hand, it can be crystallized and then cooled. It turns out that in the neighborhood of absolute zero, the disorderly mass has a distinctly higher entropy than does the crystal. This is as we should like it. Candor obliges me to mention that there are people who contend that we ought not to talk about the entropy of the amorphous glycerine at all, on the ground that this is a supercooled liquid in an essentially unstable state. So for that matter are white tin and monoclinic sulphur unstable at the low temperatures, and I should think that anyone who impugns the result for glycerine would have to impugn the results for those two also. However that may be, S_0 *as defined by our equation* is certainly bigger for disorderly than for ordered glycerine.

FIG. 1. A lattice of unoccupied sites.

FIG. 2. A lattice populated in an orderly way with two kinds of atoms (binary alloy with superstructure).

Now let us think about compounds. As a matter of fact I have already included them without mentioning them, for some of the diatomic and triatomic gases which obey the rule are compounds. This shows that there does not need to be anything disorderly, in the present sense, about a crystal built up of two or more kinds of atoms instead of one. I give you another example recently studied with great care, that of lead bromide, $PbBr_2$. Let us suppose that at a certain temperature T_t and under a certain pressure p, there were a reversible transition between the elements lead and bromine on the one hand, the compound lead bromide on the other; and suppose that L_t represents the heat of combination in this transition. Then on the one hand we could add up the integrals $\int_0^{T_t}(C_p/T)\mathrm{d}T$ for lead and for bromine; and on the other hand we could take the corresponding integral for lead bromide, and add to it the quantity L_t/T_t. If the two sums were equal the compound would be just as orderly (at the absolute zero) as the elements, and if the two sums were different this would not be the case. Now the experiment is not so simple as this, not by far! Reversible transitions of this

FIG. 3. A lattice populated in a disorderly way with two kinds of atoms (binary alloy free of superstructure).

kind have to be performed (if they can be performed at all) in galvanic cells, and one has to measure the electromotive force as well as the heat absorption, and calculate something called the "free energy," and go through quite a lot of trouble before getting to the equivalent of the starting point of the reasoning which I just sketched. However, the reasoning is sound and the experiments are good, and the result is that lead bromide at the absolute zero has the same entropy as have the lead and the bromine by themselves, which common value presumably is zero.

I repeat, then, that there are crystalline mixtures of atoms at the absolute zero which display no "disorder" at all, in our tentative sense of the word "disorder." But there are also crystalline mixtures which *do* show disorder; and to illustrate these, I will go over into metallurgy. Incidentally, I will also go away from the absolute zero, but this is not really important. The only reason for going to absolute zero, as the reader has probably divined, is to eliminate the "disorder" introduced by thermal agitation, which incidentally is one of those things that almost anyone would call disorder. If we can make proper allowance for it at a higher temperature, that comes to the same thing as eliminating it.

Figure 1 represents what may be called a "square lattice"—a section in two dimensions of a cubic lattice. The dots of this lattice do not stand for atoms—not yet. They stand for the positions marked out for atoms to occupy, and are technically called "sites." Figures 2 and 3 represent these sites shared out among two kinds of atoms, symbolized, respectively, by circles and by crosses. The two kinds are shown in equal number, so that each occupies one-half of the sites. This is true of each of the figures, and nevertheless there is a difference between the two "arrangements." In Fig. 2 I have distributed the sites among the two kinds of atoms in a way which anyone, I believe, would call an "orderly" way; in Fig. 3 I have done my best to distribute them in a way intended to look disorderly. If the type of order and disorder which is here exemplified is the type which is correlated with entropy, then Fig. 3 represents a higher degree of entropy than does Fig. 2.

It will probably be of advantage to become more specific. A mixture of equal numbers of two kinds of atoms is known, in metallurgy, as an "equimolecular binary alloy," or a "50–50 alloy." A good example of such an alloy, for present purposes, is afforded by copper and gold. The alloys of these two metals crystallize on a cubic lattice, whatever the proportions in which the metals are mingled and even if one is entirely

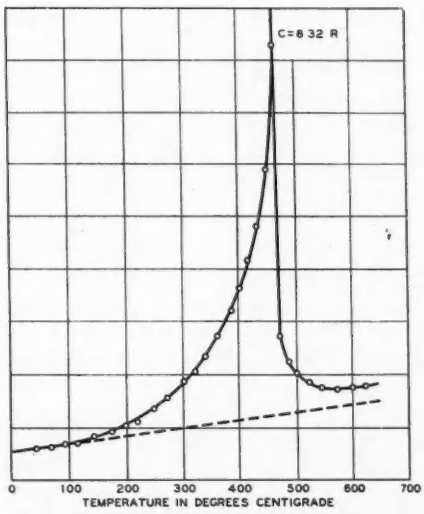

FIG. 4. Specific-heat curve of an equimolecular copper-zinc alloy (beta-brass). After H. Moser, by way of the article of Nix and Shockley cited in footnote 7.

absent so that the other is pure. The 50–50 alloy is denoted by the symbol CuAu. The crosses of Figs. 2 and 3 may therefore be taken as standing for copper atoms and the circles for gold, or vice versa.[5]

But is there any way (other than what our theory of entropy may yet afford) of testing whether a given piece of CuAu alloy has its atoms arranged after the manner of Fig. 2 or after that of Fig. 3? There is indeed; this is an important fact. If a beam of x-rays passes through or is reflected by the alloy, it is diffracted, and the manner of its diffraction is affected by the arrangement. I cannot treat this interesting subject without diverting the course of the argument excessively; but it may be said that as the experiment is ordinarily set up, a line spectrum appears on a photographic plate, and in the case of Fig. 2 it has more lines than in the case of Fig. 3. The extra lines are known as "superstructure lines," and are the sign of what metallurgists call "superstructure" though I have been calling it "order." By their presence or their absence, then, these lines show us whether order is partly present or entirely absent; and from their intensities it is even possible to draw inferences about the degree of disorder, but this is too deep a topic for the present article.

Now if I could say that the arrangements of Fig. 2 and Fig. 3 can both exist at the absolute zero and can be reversibly transformed one into the other, then my exposition could run parallel to those of previous pages. However, this cannot be said, or at any rate it does not describe any actual experiment. The ordered arrangement prevails at low temperatures, the disordered arrangement occurs when the alloy is hot.

If I could say (i) that the transition from order to disorder occurs suddenly and completely at a temperature T_t, and (ii) that there is no other cause of increase in entropy as the alloy warms up than is afforded by the transition, then the experimental facts would be easy to describe and interpret. If the inflow of heat were to be plotted against the temperature of the alloy, the curve would consist of a sharp narrow peak rising from the axis of abscissae at T_t, and the ratio of the area under this peak to T_t would be the entropy difference between disorder and order. This, however, is a bad oversimplification, and must be amended in two ways.

In the first place, the transition from order to disorder occurs not suddenly and completely at a temperature sharply defined, but slowly and gradually over a range of temperatures covering many hundreds of degrees. This the x-rays attest, and so do various properties of the alloy less obviously controlled by the degree of order. Therefore, instead of a needlelike peak in the curve of heat inflow *versus* temperature—or let me say from now on, the curve of specific heat *versus* temperature—instead of a needlelike peak in the specific-heat curve, we must expect a very broad maximum.

In the second place, the disorder exemplified in Fig. 3 is not the only kind of disorder which increases as the alloy warms up. There is in addition the most familiar kind of all, the one called "thermal agitation"—incidentally one of the kinds which answer most nearly to the popular conception of disorder. As the temperature of the alloy rises, heat must flow into it for increasing the energy of thermal agitation and the entropy of what I may call thermal disorder. This effect must be distinguished from the one in which we are interested. The way of distinguishing it is to assume that for this 50–50 alloy, the effect is the mean between the effect for pure copper and the effect for pure gold. The specific-heat curves for these two pure metals exhibit only the inflow of heat required to build up the thermal agitation and the entropy of thermal disorder, for when there is only one kind of atom there is no possibility of the sort of disorder exemplified in Fig. 3. We plot the curve midway between these two, and in Fig. 4 (for which I have borrowed an excellent graph of the data for an equimolecular CuZn, instead of CuAu, alloy) it is seen as the dashed curve.

Over the dashed curve of Fig. 4 rises the "hump" of the actual curve for the equimolecular alloy, which merges into the dashed curve on its

[5] It is not so easy as one wishes to pass from the oversimplified two-dimensional lattices of Figs. 2 and 3 to the actual three-dimensional lattices. However, in the case of CuAu it is possible (in imagination) to draw a plane through the three-dimensional lattice in such a way that the arrangement of the two kinds of atoms in the plane resembles Fig. 2 or Fig. 3, according as there is order or disorder.

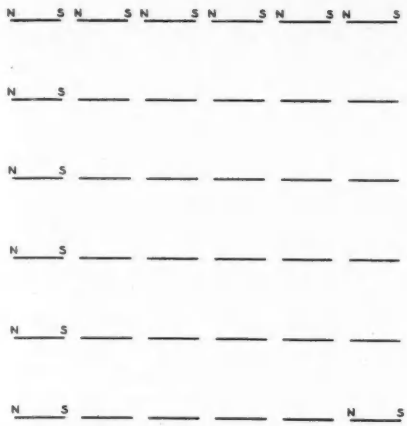

FIG. 5. A lattice populated in an orderly way with magnetic atoms of a single kind (domain in state of magnetic saturation).

two flanks, as it should. This hump is the visible sign of the transition from the order of Fig. 2 to the disorder of Fig. 3. The area between it and the dashed curve is the measure of the total heat required to convert the order into the disorder, technically called the "configurational energy." We cannot find so easily a measure of the "configurational entropy," the excess of the entropy of the disorderly arrangement over that of the orderly one. We must divide the area between dashed curve and hump into vertical strips of infinitesimal breadth, divide the area of each strip by the corresponding temperature, and sum the quotients. A better way of doing this would be to plot the two curves against ln T, the natural logarithm of the temperature, in which case the area between them would be the configurational entropy. The difference in entropy between the disorder of Fig. 3 and the order of Fig. 2, between the absence and the presence of a superstructure, can thus be measured.

Is there a theory which professes to be able to evaluate this difference, and so presents itself for test? Here I must touch very lightly on a very complicated subject; for the problem is nothing less than that of contriving a numerical measure for this quantity vaguely called "disorder," and not just any measure either, but one which can be correlated with the entropy observed.

To illustrate an attempt at finding such a measure, I refer the reader back to Fig. 2, and ask him to regard the circles and the crosses as marking two distinct lattices, which interlock to form the lattice of the crystal. "Perfect order" is taken as occurring when there is an atom of one kind at every circle and an atom of the other kind at every cross. "Perfect disorder" is taken as occurring when half the circles are occupied by atoms of the one kind and half by atoms of the other, the same being true of the crosses. The number W' of ways in which perfect order can be realized is calculated by the methods of statistics, and so is the number W'' of ways in which perfect disorder can be realized. The corresponding entropies are taken to be k ln W' and k ln W'', k standing for the Boltzmann constant; this is the general procedure in theorizing about entropy. The excess of the latter over the former should be equal to the measured value of the configurational entropy. Actually, in the few cases tested, it is of the right order of magnitude but rather too large: one may infer that even at the highest temperature attained the disorder is not yet perfect, or else that we have not yet found quite the right measure of disorder. I relegate the calculation to a footnote.[6]

Even if the agreement here were perfect, one would want more from a theory: One would want an explanation of the peculiar and characteristic shape of the hump of Fig. 4 and all the others of

[6] Suppose N sites altogether, $\frac{1}{2}N$ being on the cross lattice and $\frac{1}{2}N$ on the circle lattice, and N atoms of which $\frac{1}{2}N$ are of kind *1* and $\frac{1}{2}N$ are of kind *2*. In the state of perfect disorder as above defined, the $\frac{1}{2}N$ sites of the cross lattice will be distributed half-and-half between $\frac{1}{4}N$ atoms of kind *1* and $\frac{1}{4}N$ atoms of kind *2*. Now we play what in various articles I have called "the game of balls and baskets." The balls stand for the sites, and there are two baskets marked *1* and *2*. The question is: In how many different ways can $\frac{1}{2}N$ marked balls be thrown into two marked baskets, it being required that equal numbers shall be found in each basket after the casting is over, and two ways being defined as different unless each of the baskets contains exactly the same balls after either casting as after the other. The answer is $(\frac{1}{2}N)!/(\frac{1}{4}N)!(\frac{1}{4}N)!$. The same question is asked for the circle lattice, and receives the same answer. The two answers are multiplied together, since any distribution on the one lattice may coexist with any on the other. The product is the quantity W'' mentioned in the text; its natural logarithm is N ln 2; the entropy of perfect disorder is inferred to be Nk ln 2. In the case of perfect order, with only one kind of atom on either lattice, there is only one basket and W' is unity, so that ln W' and the entropy of perfect order are zero. The difference between the two is accordingly Nk ln 2, and this is the value presented by the theory for the configurational entropy. Actual values run some three-quarters as large. (The "Stirling approximation" is used in evaluating the logarithms of the factorials.)

its ilk, with the concave-upward arc on its left, the sharp peak, the cliff or sudden fall on its right and the eventual very gradual approach to the dashed curve. As the temperature ascends the disorder mounts gradually at first, then faster and faster, then almost suddenly becomes almost complete, and yet a residue lingers which is not eliminated until the temperature goes higher yet. (By the way, the temperature of the cliff or sudden fall is called the "Curie point of order," though Pierre Curie died long before it was observed; I will presently account for this.) To explain such details is beyond the scope of thermodynamics: The theorist must make postulates about the forces between the atoms, and seek also for a better measure of disorder than the one I have already quoted. These difficult theories were being ardently developed just before the war, and should be resumed hereafter. The shape of the curve has been explained in a general way, but many a detail remains to be interpreted.[7]

Now we turn to an example of order and disorder which will be more familiar to many. To introduce it, I populate the lattice-sites of Fig. 1 with atoms of a single kind, represented in Fig. 5 by bars or dashes each marked with N at one end and S at the other. It is hardly necessary to say what the reader has already divined, that this is intended to portray a *magnetic* substance. The best imaginable order is shown in Fig. 5, while Fig. 6 is an attempt to exhibit a high degree of disorder. But Fig. 5 is evidently meant for a portrayal of a magnetic substance in the state of "saturation," while Fig. 6 represents the same substance quite without magnetic moment, or "demagnetized" as the customary term is. Here we have a kind of disorder that is correlated with something which should be easy to observe and to measure, to wit, the magnetic moment of the substance. The bigger the magnetic moment, the smaller the disorder.

What follows is believed to be true of all ferromagnetic substances. Of these iron is the most familiar, but it happens that the best of the relevant experiments have been done upon nickel; I therefore take nickel as my example.

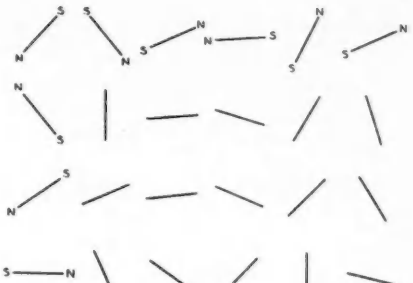

FIG. 6. A lattice populated in a disorderly way with magnetic atoms of a single kind (domain in state of demagnetization).

I may now be expected to say that whenever a bar of nickel is magnetized (or demagnetized), it passes from the condition shown by Fig. 6 to the condition shown by Fig. 5 (or reversely), and its entropy goes down (or up) by a notable amount which may be calculated by an analysis of the figures. This, however, is not true. The behavior of nickel, as of other ferromagnetic substances, is confused by the existence of "domains." Here again is a topic too extensive to be treated in this article! I have reviewed it elsewhere,[8] and can here say only that a bar of nickel is a sort of three-dimensional mosaic of small regions known as "domains" (*not* single crystals!) any one of which may be represented by such a picture as Fig. 5 or 6. The magnetic moment of any such domain, P let me call it, is the vectorial sum of the magnetic moments of all of its atoms. This it is which is a measure—one kind of measure—of the disorder in the domain. Complete disorder in any domain entails a value of zero for the P vector of that domain. But when a bar of iron at room temperature is apparently demagnetized, that does not mean that the P vector of each and every domain is zero! There is a considerable degree of partial order in each domain, and the P vectors are not

[7] See the article by F. C. Nix and W. Shockley, "The order-disorder transformation in alloys," Rev. Mod. Phys. **10**, 1 (1938).

[8] "The theory of magnetism," Bell Sys. Tech. J. **15**, 224 (1936). For a brief description of the domain theory, see R. M. Bozorth, Am. J. Phys. **10**, 73 (1942).

zero; but they are pointing in all directions at random, like the dashes in Fig. 6. At the other extreme, when the bar of iron at room temperature is apparently magnetized to saturation, that does not mean that perfect order prevails in each and every domain! The order within the domains is very little changed from the case of apparent demagnetization, but the P vectors are all pointing parallel to one another, like the dashes in Fig. 5.

So, the apparent magnetization of a bar of iron gives no indication—or I should say more carefully, no immediate indication—of the state of disorder prevailing within the domains. What is needed is a set of experiments on a single domain all by itself; but alas, nobody has succeeded in making one! Fortunately there is one type of experiment on a good-sized piece of nickel which, it is believed, yields exactly the same results as it would if the whole piece were a single domain; and more fortunately yet for the present purpose, it is the same sort of experiment as we have just been studying in the case of the binary alloy!

What nickel exhibits, in the way of the curve of specific heat against temperature, is shown as curve A in Fig. 7. There is evidently a marked resemblance between it and the curve of Fig. 4, and the "cliff" is especially distinct. Near the abscissae of the cliff lies the "Curie point," beyond which nickel is no longer ferromagnetic; the honor done to Pierre Curie for his work on ferromagnetism by this name has now been enhanced by applying the name to the order-disorder transition which we lately considered.

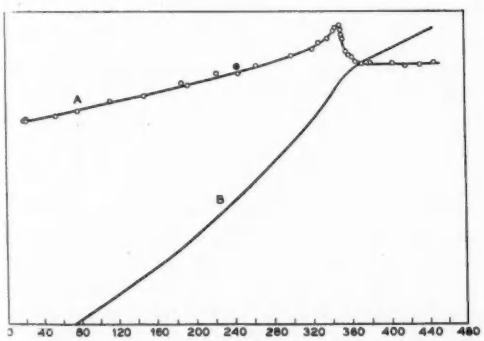

FIG. 7. Specific heat (curve A) and resistivity (curve B) of nickel plotted as functions of temperature. (After Ahrens.)

This curve, then, is the testimony of metallic nickel to a particular type of disorder within itself. It is a type which rises as the temperature rises, requires the inflow of heat to increase itself and is associated therefore with a rise of entropy. It is a kind which is due to the polar or vectorial character of the magnetic atom, and is a disorder in the orientations of the little bars or arrows by which the atoms must be represented. When it is zero at or near the absolute zero, each domain is magnetized to saturation and is portrayed by Fig. 5. If the P vectors are wheeled into alignment, the whole piece of iron is magnetized to true saturation. When the disorder is complete, each domain is without magnetic moment and is portrayed by Fig. 6. There are no P vectors any more to be wheeled into line by a modest magnetic field: the iron has ceased to be ferromagnetic.

Sometime about 1920 Pierre Weiss (he to whom we owe the notion of domains, and much else in our knowledge of ferromagnetism) was applying magnetic fields of various strengths to samples of nickel at temperatures near the Curie point. This point for nickel being near 356°C (629°K) the samples had to be in a furnace, and the temperature of the metal had to be accurately measured. There was a thermocouple squeezed against the nickel mass and connected in series with a galvanometer. When Weiss applied a strong magnetizing field, he saw the coil of the galvanometer jump as though it were reporting a rise in the temperature of the nickel; when he annulled the field, the galvanometer implied a sudden cooling of the metal. Not only did Weiss not anticipate this, he could not at first believe it when he saw it. But his apparatus was not deceiving him; he had discovered what he soon entitled "the magneto-caloric effect." This we now must try to interpret in terms of order and entropy.

As I said at the beginning of this discourse, a rise of entropy dS, provided it occur reversibly (!), is revealed by an inflow of heat $T\,dS$ into the system where it is located. This heat comes to the system out of the rest of the world, or out of the "environment" as I will temporarily say. As I have been saying throughout the middle of the discourse, a rise in disorder is associated with a rise in entropy. Therefore, when disorder increases in a system (or decreases) heat may be

expected to flow in (or flow out). More precisely one can hardly speak, for to predict the amount of the inflow or outflow one would need (*a*) to be sure that the process is reversible, (*b*) to know an adequate numerical measure of the disorder and (*c*) to know the relation between this measure of disorder and the associated entropy. These conditions we cannot fulfil, and so the qualitative statement is all that can be made.

The qualitative statement quite agrees with the facts as Weiss observed them. His thermocouple grew warmer when the magnetizing field went on, cooler when the field went off. However, we cannot so easily make the demarcation between "system" and "environment." Everything indicates (so far as I know) that the piece of nickel itself grows warmer with magnetization, cooler with demagnetization: as if the magnets of Figs. 5 and 6 were one system, and the metal were another system serving as the environment of the first.[9] Perhaps the most plausible way of conceiving of this is to identify the "environment" with the thermal agitation of the nickel atoms. This last it is, which takes up (or gives off) the heat which must be transferred in order that the entropy of the system of atomic magnets may fall (or rise). Whether or not this is the best way of conceiving what happens, it is a fact that a magnetizable metal is warmed by magnetization, cooled by demagnetization. The fact has been verified for the major ferromagnetic metals and several alloys, and studied in detail. There is no doubt that the phenomena are due to the change in the degree of disorder in the domains, and not to any other influence of the magnetic field. As the process may occur so rapidly that during it there is little exchange of heat between the metal and the outside world, and this little appears to be irrelevant, it is called "adiabatic." The cooling in particular is called by the name of "cooling by adiabatic demagnetization;" but as this is altogether too cumbrous, I will refer to it hereafter as "magnetic cooling."

The magneto-caloric effect was destined to lead on to a very remarkable consequence; indeed, nothing less remarkable than the attainment of the lowest temperatures ever so far reached, perhaps the lowest which the human race will ever succeed in producing.

So far as can now be determined, this was not foreseen at the time of the discovery, and no wonder! The fall of temperature which Weiss obtained with nickel was less than a degree, and to get it he had to heat the metal to 629°K—not a promising way to start reaching for the absolute zero. Obviously a seeker for temperatures of unprecedented depth would want to start from the lowest level otherwise attainable, by putting the metal into liquid helium rather than into a furnace. But by so doing he would frustrate his hopes at once. Magnetic cooling, be it remembered, depends on our ability to convert disorder into order, so that then we may stand by while the order reverts to disorder. At the temperature of liquid helium, the order in the domains of nickel or in those of any of its familiar congeners is always practically perfect. We cannot magnetize them any more strongly than they are already magnetized *per se*, and since it is impossible to undo what we have not done, we cannot even set the stage for the operation of magnetic cooling.

What is required, then, is a substance having magnetic atoms or molecules, which are naturally in a state of high disorder even at the temperature of liquid helium. Such substances exist. They are of the type called "paramagnetic" instead of the type called "ferromagnetic," but this is an advantage to the student, as it means (among other things) that he does not have to worry about the multitude of domains—or, otherwise expressed, any sample of the substance is just one single domain. One of these substances is gadolinium sulphate, and another is chrome-iron alum. If a sample of either is put into liquid helium it assumes the temperature of the liquid, but its elementary magnets are still randomly oriented after the fashion of Fig. 5. If now it is subjected to a powerful magnetic field, it approaches the state of order exhibited in Fig. 6. Simultaneously it grows warmer, but the heat is spent in vaporizing some of the helium, and then there is an ordered flock of elementary magnets at the original temperature. Now let the magnetic field

[9] A similar paradox is offered by the closely-allied gyromagnetic effect. When the atomic magnets suffer the change in orientations suggested by the comparison of Figs. 5 and 6, the resultant of their angular momenta changes, and this is manifested by an equal and opposite change in the angular momentum of the piece of metal. The metal is the mechanical environment of its own elementary magnets, just as it is the thermal environment.

be annulled. The disorder returns, the substance cools, and for the brief interval of time which elapses before inflow of heat from the helium rewarms it, we have a lower temperature than any heretofore known! This effect was foreseen independently by Giauque in California and Debye in Switzerland, was promptly verified by the former, and has been verified times without number in the twenty years intervening. Magnetic cooling affords to us now the sole and the standardized way of reaching below the temperature of liquid helium.

The purpose of this article has been to establish a connection between the subtle and difficult notion of entropy and the more familiar concept of disorder. *Entropy is a measure of disorder*, or more succinctly yet, *entropy is disorder:* that is what a physicist would like to say. I have chosen several examples in which entropy is linked with something which clearly looks like disorder, in the customary meaning of this word. Probably, however, it has not escaped the reader that these examples were deliberately chosen with that end in view, and that anyone with an opposite intent might find examples in which a rise in entropy is not matched by an increase in anything that would normally be called disorder. A gas at a constant temperature, for example, suffers a rise in entropy when it expands: would anyone spontaneously say that a gas in a two-litre bottle is more disorderly than the same gas at the same temperature in a one-litre bottle?

We cannot therefore always say that *entropy is a measure of disorder* without at times so broadening the definition of "disorder" as to make the statement true by definition only. This, however, is the normal career of any word which is taken over from the popular or the literary language and introduced into the language of science. Nearly all such words suffer distortion in the course of becoming scientific words; and we should probably be grateful that the word "disorder" needs to suffer no more distortion than it does, in the process of adaptation to the statement that entropy is a measure of disorder.[10] Quantum mechanics has already demanded the privilege of redefining disorder; I can only hope that we shall be able to understand the definition when it is given. It will have to account for the unwelcome fact that *liquid* helium has zero entropy at absolute zero.

I conclude with three paragraphs, one pertaining to an error frequently made concerning entropy, the second to the meaning of "zero entropy," and the third containing a restatement of the meaning of Eq. (3).

A common error consists in assuming that there is a law of Nature to the effect that the entropy of any body, object or system can only increase or at the least remain constant, but never may decrease. This is true only of a "closed" system, that is, a system which has no transactions with the rest of the world. For any system which is not closed, the assumption is not true. The correct statement for such a system is, that its entropy may increase or remain the same or even may decrease, but if its entropy does decrease, then the diminution must be offset by an equal or a more than equal rise in the entropy of the rest of the world. Conceivably the aging man, the burning house or the eggs which the cook is scrambling (to take my first illustrations) may not be undergoing a rise of entropy at all: the rise of entropy may in each case be localized in the rest of the world. This is conceivable, but it seems highly unlikely, and so I do not repudiate my illustrations. Assume that they are good illustrations, as is likely, and that the entropy of the system does increase in each case; then conceivably we might make the man grow younger, the house rebuild itself or the eggs unscramble themselves, if we could arrange for a countervailing rise of entropy in the rest of the world. This is thermodynamically possible; but unfortunately there is a very long step from "thermodynamically possible" to "technically possible" and I am afraid that we shall never make it in these cases. What is perhaps more important is to realize that when we witness disorder changing of itself into order, as when a liquid becomes a crystal or soil becomes a plant, we should not leap to the conclusion that the second law of thermodynamics is being overthrown. The entropy may be going down in the part of the world at which we are looking, but it may be

[10] Once when I was lecturing on this topic before the physics seminar of a prominent university, a mathematician told me that for a gas at a constant temperature the disorder "obviously" increases with the volume, because the atoms have more room in which to move about. This gentleman had already made a considerable amount of unconscious progress in redefining the word "disorder" to suit the demands of physics.

going up somewhere else by even more, and presumably it is; if there is anyone who doesn't think so, the burden of proof is on him.

As to the meaning of "zero entropy," it is best apprehended by thinking once more of order. *Zero entropy corresponds to perfect order.* To this I now add by way of amplification: *If two or more types of disorder coexist, each makes to the total entropy a contribution of its own, which vanishes when it vanishes.* Indeed a system may have perfect order in one respect and not in another. A domain of a piece of iron at room temperature is already close to the state of perfect order of the elementary magnets exhibited in Fig. 5, and so its entropy of magnetic disorder is relatively small; but it has still a good deal of the kind of disorder which is called thermal agitation, and the corresponding portion of its entropy is by no means negligible. On an earlier page I spoke of a formula derived from the new statistics, which when combined with data of experiment leads to the conclusion that the entropy of a crystalline solid is zero at the absolute zero. This formula takes account of certain types of disorder, including (when the formula is properly written) one which has not even been mentioned in these pages: disorder of the orientations of the atom-nuclei. Had this type not been known and included, the imperfect formula would have undervalued the entropy of the gas, and as a result the entropy of the solid at the absolute zero would have been found to be less than zero. Such a result would have seemed to make nonsense of our theory of entropy, and yet it would simply have meant that we had overlooked one of the natural types of disorder. Such an adventure may yet befall us through the overlooking of a type of disorder not yet even divined.

Now finally I can state the significance of the basic equation (3) in an accurate way. I will speak of entropy as consisting of "portions," each portion corresponding to a particular type of disorder. Account must also be taken of the fact that actually we never get down to the absolute zero, so that in practice the lower limit of the integral in (3) is not zero but some temperature T' higher than zero. Let us suppose that in some particular case the sum of the bracketed terms in (3), with T' set as lower limit to the integral, is sensibly equal to S_{th}. If this is not a mere coincidence, it means that we have correctly evaluated every portion of the entropy which practically vanishes when the temperature sinks to T', with the possible exception of portions which remain sensibly constant over the entire temperature-range covered by the integral.

The Use of Physics and the Methods of Physics in Archaeology

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IT is never surprising to a physicist that the principles and methods of physics should be found useful in other sciences. Physics is an exact science, the most fundamental and diversified of all the exact sciences, and its principles are thus basic to an understanding of all natural phenomena.

Archaeology, a branch of the larger science of anthropology, is one of the social sciences. As such, it cannot be called an exact science. It is often difficult and sometimes impossible to give exact quantitative expression to its fundamental concepts. Because life processes enter into most of the phenomena investigated by archaeology, there is always a degree of uncertainty in interpretation of the relation of cause and effect. To one trained in the methods of physics, it may come as a distinct shock to find, because of this disturbing factor of life, that in interpreting archaeological data one cannot always assume that like causes will produce like effects.

Naturally, archaeologists seek by every means at their command to increase the validity of their conclusions and to extend the scope of their generalizations in both time and space. This they do by calling to their aid the other natural sciences, and especially the exact sciences. The contributions of statistics and of chemistry to

archaeology are frequent and well known. Archaeology often draws heavily on the sciences of geology, osteology and paleontology for the basis of its conclusions.

THE METHODS OF PHYSICS IN ARCHAEOLOGY

Perhaps the most important contribution which physics has made to archaeology is its scientific method, which it offers the archaeologist as a model for his thinking. While the scientific method is not the sole property of physics, yet because physics is the most basic of all the natural sciences, its processes and its technics probably illustrate the scientific method most completely and simply.

For our present purposes we may briefly define the scientific method as a process by which one reaches satisfactory conclusions relative to natural phenomena through the following steps:

(i) Careful observation of all related phenomena.
(ii) Complete recording of all observable data.
(iii) Deduction of a suspected relationship between cause and effect on a basis of observed data.
(iv) Experimentation—that is, controlled observation—to test the validity of the suspected relationship.
(v) Quantitative determination of the phenomena involved.
(vi) Statement of the relationship demonstrated or of the conclusions drawn.

Archaeology has always followed this process, the so-called scientific method, as far as it was possible to do so. Steps (i), (ii) and (iii) have long been a part of archaeological technics. Not until very recent times has step (iv) been regarded in archaeology as a *necessary process* in the drawing of valid conclusions. It is not always possible in the varied phenomena investigated by archaeology to reproduce causes under precise conditions. Controlled experimentation is often quite difficult. The urge in modern archaeology to carry on such experimentation wherever and whenever possible and thereby to increase the validity of its conclusions comes doubtless from the example set by the natural sciences, particularly physics.

Step (v), the quantitative determination of the factors involved in investigations, has only recently come to be widely attempted in archaeology. The step is always a difficult one, difficult in even the physical sciences and much more so in the biological sciences; nevertheless, it is being attempted today in archaeology. As a result, there are often to be found in modern archaeological reports quantitative data, arranged in tabular form, which provide the basis for statistical conclusions. Today the manifestation of a cultural complex is described not in words only, but by a "trait list." A "trait" is a unit expression of cultural manifestation. A cultural complex at any site is described by a complete list of its traits. This list may be compared to a similar list of traits from another site, or manifestation, and their similarity or possible kinship expressed by a percentage correlation. Frequently complicated statistical conclusions presented in mathematical formulation adorn the pages of archaeological papers, and they are often accompanied by bar charts, graphs or other visual aids to show functional relationships between variables, or "trends" of a variable in time or space. Thus, one cannot doubt that in modern archaeological research physics has, by its example, set standards for formulating dependable, valid conclusions, by use of the scientific method. Archaeology has accepted these standards, has more completely adopted the scientific method and is rapidly learning to use all of its technics.

USE OF THE PRINCIPLES OF PHYSICS IN ARCHAEOLOGY

If one seeks in archaeology the use of the principles of physics, he may be disappointed to find that frequency modulation, alpha-particles, cosmic rays, quanta and other concepts of modern physics have not as yet penetrated the boundaries of the archaeologist's thinking. Whether the latter has been screened by his own particular "Heaviside layer," or has an "uncertainty principle" of his own quite as great as he can bear, need not concern us here. The fact is, modern physics has as yet had very little effect on archaeological technics, although this situation may be very greatly changed in the future. However, the elementary principles of classical physics are repeatedly called into play to clarify observations, or to correct or validate archaeological conclusions. How the principles are used can perhaps best be presented by specific illustrations.

One very important type of archaeological

site in the Southeastern United States is the deep shell mound. These great mounds, made largely of shells of river mussels, are found on the banks of many of the large rivers. They often cover several acres in area and are sometimes 25 ft deep. They represent the accumulated village debris of prehistoric peoples living for centuries on a single spot on a river bank. Naturally such people were not at all interested in leaving any record of their presence or, for that matter, a record of any other natural phenomena. Obviously over the centuries one might expect that cultural stratigraphy would be found to occur at such sites; that is, early peoples laid down the lower levels of the midden, and later peoples in succession contributed to the upper levels, each in their proper chronological order. The people of each age cast out in the debris artifacts made by their own hands and characteristic of their own time. Thus they "built" cultural stratigraphy into such mounds.

It is usual to investigate such great middens by trenching from top to bottom, thus exposing a vertical wall, or "profile," which has in it an archaeological history of the site in chronological order. Now the many specimens of river mussels gathered by prehistoric man may be, for our purpose, separated into two groups, the bivalves (pelecypods) and the univalves (gastropods). The Indians gathered these shell fish, any kind they could get, ate the fish and threw away the shell along with other debris of the camp grounds. All such material would thus be expected to be thoroughly mixed and in a shell mound profile the mixed univalve and bivalve shells should appear in complete disorder.

In excavations on the Tennessee River, on several occasions it was observed that the pelecypods and other camp debris would be mixed but the gastropods would appear in the profile in long thin lines indicating thin layers of gastropod shells in the midden. This phenomenon observed by conchologists and by anthropologists was interpreted as due to a "seasonal variation" in the abundance of certain species of mussels in the river. That is, when bivalves were abundant and easy to get, the Indians ate them almost exclusively, but when gastropods became abundant they in turn were eaten, and the remains scattered on the surface of the midden; thus these shells would appear in alternate layers. This simple interpretation seemed at first to offer a reasonable explanation of observed fact, which has been frequently noted. However, when the "history" of the profile came to be studied, and every item of its construction critically examined, it appeared that the "seasonal variation" of the food supply came at *long intervals*, not merely years, and such variations grew *less frequent* as the midden became deeper. Also, no zoologist had ever observed any such seasonal variations in mussels found in the river at the present time and knew of no reason why any such variation should occur. Further, while the old surfaces of the midden which could still be observed were irregular and never horizontal, these lines of gastropods in the profile were *always* exactly horizontal as determined by stadia rod and transit level. Finally, after much careful investigation and experimentation, it was proved that in times of high water in the river, quiet back water from the creeks would cover the shell mounds and while the bivalve shell, now in half shell, would be left in position, the gastropod shells, because they were hollow and could catch a bubble of air, were floated. This differential flotation automatically sorted out most of the gastropods and left them in thin layers. Thus Archimedes came to the aid of the archaeologist, and differential flotation was found to have left a chronological record of the high water in the river. By the application of a principle of elementary physics, an unexplained "seasonal variation" in food supply gave way to another phenomenon—high flood stage in the river—known to be rather frequent, but irregular.

As the result of study of and experimentation with rock fracture under different conditions, archaeologists can distinguish at a glance fractures caused by freezing, by percussion or by sudden cooling of stone after heating to high temperature. These different types of fractures are caused by different types of stresses. Thus, by observation on its form of fracture, one can determine today to what condition a particular sample of stone must have been subjected hundreds of years ago.

By careful observation of the earth profiles in earth mounds it is possible to determine whether the earth carried upon the site to build the mound

was moist when transported and remained as a single mass, or was dry and crumbling and hence spread and mingled with other loads. This physical difference, a matter of simple observation, often has important consequences in interpreting events at a particular site.

Some years ago, a well-known archaeologist of the old school reported finding artifacts of copper in association with a cremated burial. He stated that the fires of cremation at the spot had been long continued, and very hot, so hot that the copper artifacts lying one upon another had adhered together as a result of melting. A later archaeologist, trained in experimentation, showed that copper could not have been melted in open wood fires of the kind under consideration, and that the copper artifacts adhered together as a result of copper salt incrustation. Thus a knowledge of the melting point of copper and of the temperatures attainable in wood fires in the open laid the basis for correct interpretation. The final conclusion in this case was that the cremation as found was a redeposit of cremated human remains, burned elsewhere, that the copper artifacts had merely been laid on the remains, and that, far from having been subjected to very hot fires, they had not been in the fire at all; there had actually been no fire at the site where the remains were found.

Instances such as these, in which a knowledge of simple physical principles came to the aid of the archaeologist and led to correct conclusions, could be multiplied indefinitely.

It should be remembered that not only does the archaeologist use physical principles to discover the relations and facts of prehistory, but prehistoric man was himself something of a physicist. Early man survived by his study and comprehension of natural laws, as is well illustrated in his manufacture of tools. His working of flint, first by percussion and later by pressure fracture, not only made it possible for him to survive but gave him an advantage by which his future cultural progress was made possible. The grinding of stone to a cutting edge shows he had learned that the proper angle of surface contact was determined by the material and the task to be accomplished. The form and design of certain house types shows he had mastered the fundamental ideas of static stresses in architectural design. His gradual development of the processes for the working of stone, bone, shell and metal shows that he had been driven, perhaps unwittingly, to understand the proper application of force and to follow natural law to his own advantage.

He used the forces of buoyance in his canoes; he so made them, sat in them and loaded them as to show that he had some knowledge of the laws of equilibrium and stability. His production of fire by friction with the fire drill required a fairly close adherence to a technic which early man had discovered and passed on to later generations.

His use of the force of elasticity as a means of propulsion of projectiles was a triumph of invention. Like most inventions, even of our time, it was at first quite crude, the final forms being the result of long ages of improvement and development.

In most of North America the bow was introduced comparatively late in prehistoric times. Before the bow, man used first his own arm to throw a spear. Later he used a "throwing stick" or atlatl for increased efficiency. Such atlatls were broad wooden staves some 26 in. long. In use their effect was to lengthen the forearm of the thrower, thus increasing the speed of the projectile, with corresponding increase in range and penetrating power. It has recently been found that many prehistoric peoples employing the atlatl were not content to use a simple stave of wood with a hook at one end as a throwing stick to assist in casting the projectile; but in order to make it a more effective instrument, they attached to the wooden stave various forms of weights made of stone, galena, barite, shell or slate. These atlatl weights surely increased the effective moment of inertia of the implement in the act of throwing, and hence the speed of the projectile, with all attendant advantages. Again, by feathering the shaft, the projectile could be made to spin. In these developments prehistoric man was learning mechanics the hard way, but he had already begun to see a connection between energy of translation and that of rotation, and had learned to depend on the application of moment of inertia in projecting missiles.

The archaeologist is trained to regard an

archaeological site as the historian does a unique historical document—as an "original document, or source" of information. There is, however, this very fundamental difference; the historian can read his document, make notes, think things over, draw his conclusions, and later he may go back and read the original document again. The archaeologist reads the record of his prehistoric document only once; for as he excavates the archaeological site, his original source of information, he destroys it. It can never be excavated—read—again. He must, therefore, get *all possible information* at the first reading. This emphasizes the necessity of a *complete record* during the excavation. In compiling this record, photography today plays a large part. Not only is a complete photographic record kept of the progress of the excavation, but every special feature is separately photographed in many ways. Great care and much scientific planning of a physical character must go into the products of the record. For example, mounds are not opened by trenches in any accidental fashion, but always with regard for the season of the year and the latitude of the site; the trenches are so orientated that at least once each day there will be the possibility of good sunlight on the advancing profile. The technics of photographing profiles are the result of the application of many physical principles. Often two earth structures of different density, portions of the same mound, show no color contrasts when dry; but if they are sprayed with water and allowed to stand for an hour, differential absorption and drying may produce strong contrasts and thus make it possible to photograph mound structure. Again, by using panchromatic films and filters it is often possible to photograph boundaries between different structures, which to the eye show no demarcation.

Today the archaeologist makes considerable use of x-ray photographs in the laboratory process. The size and shape of the particles of the tempering material, the aplastic, used in the manufacturing of pottery are determined by x-ray photographs. The form and extent of deep drilling in the manufacture of artifacts such as pipes and atlatl weights are often determined by x-ray photographs much more exactly than would be possible by other means. Skeletal material recovered by archaeological investigation is often found to be pathological. The study of such osteo-pathological specimens is of great interest to medical men, for in prehistoric man they have their only chance to see the effect of the ravages of disease carried to its final conclusion, unameliorated and untreated by modern medicine. Such studies of pathological material are greatly aided by x-ray technics.

The listing of the use of physical principles and the apparatus and technics of the physicist could be multiplied. One more illustration, involving microscopy, must suffice. A microscopic examination of the dust on the floor of a prehistoric habitation, which lies at the bottom of an earth mound and thus has been covered over for centuries, will reveal to the archaeo-botanist the kind of pollen grains that were blowing in the air when the house was open. Thus he can tell what plants and trees were in bloom before the house floor was covered.

Finally, since the task of the archaeologist is to make the unintentional records of the past reveal their true and complete story, he is constrained, like Sherlock Holmes, to develop a multitude of technics by which he tests, examines, probes for and extracts the truth from a complicated mass of data. The basis of every one of these technics is some fundamental principle of natural science, usually physics.

The rapid advance of archaeology in North America in the past two decades may be ascribed in no small part to a more general application of the scientific method to archaeological problems, and in particular to the use of controlled experimentation following the principles of physics, as a means of checking the validity of conclusions.

I hope that my children, at least, if not I myself, will see the day when ignorance of the primary laws and facts of science will be looked upon as a defect only second to ignorance of the primary laws of religion and morality.—CHARLES KINGSLEY (1819–1875).

Coulomb's Law and the Dielectric Constant

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ABOUT ten years ago Wilberforce[1] called attention to the fact that the insertion of μ, the permeability, into the denominator of the equation for Coulomb's law of magnetostatics was wrong. During the years 1933 to 1935 three other papers[2] appeared on the subject, all in agreement with regard to this matter. If anyone has defended the equation, it has not come to my attention.

I recently made a survey of such textbooks, published since 1935, as were readily available. Thirty-eight of these deal with the subject of magnetostatics and 21 of them, or 55 percent, used the equation in question. During the years immediately following 1935 the frequency of occurrence decreased, but more recently it has increased. During the three-year period 1937–1939 it was 39 percent and during 1940–1942, 64 percent. If the equation is in fact incorrect, the attention of physics teachers should again be called to the matter.

It is frequently stated that this equation is analogous to the corresponding equation of electrostatics. The following is a brief outline intended to show that the conditions which make the electrostatic equation valid do not obtain in magnetostatics. Let us first review and compare two methods of developing the subject of electrostatics from Coulomb's law.

One development starts with the equation

$$F=\frac{1}{\epsilon_o}\frac{qq'}{r^2}. \tag{1}$$

This equation is closely analogous to the law of universal gravitation: $1/\epsilon_o$ is a constant of proportionality that depends only on the units used, analogous to the gravitational constant G; q and q' are infinitesimal charges, analogous to the material particles of mechanics; and the finite force acting on a charged body must be computed by a double summation. The medium has no effect except that in the summation all charges must be considered, including those which are a part of the structure of the medium.

The other development starts with the equation

$$F=\frac{q_c q'_c}{\epsilon r^2}, \tag{2}$$

in which ϵ is the dielectric constant. The quantities q_c and q'_c are the so-called free charges. The charges that constitute matter are not considered, and the effect of the medium is supposed to be accounted for by the introduction of the dielectric constant.

The analogy between the subject of universal gravitation and the development which starts from Eq. (1) fails in one particular. There is only one kind of gravitating matter, and it attracts. There are both positive and negative charges, and like charges repel. This introduces a difference in sign in corresponding equations such as those for field intensity, potential and the statement of Gauss's law. It also causes a differential action of the electric field on the positive and negative charges that constitute matter, giving rise to electric polarization, which has no gravitational analog. In this connection it is necessary to introduce a second fundamental postulate, namely, that the polarization is a linear function of the intensity of the electric field. One then classifies electric charges into two categories—free charges, q_c, and charges due to polarization, q_p—and proceeds to develop equations that involve q_c but not q_p. The importance of this lies in the fact that laboratory instruments make direct measurement of the free charge. Among the equations thus developed are the equation for the surface integral of the normal component of the electric displacement over a closed surface,

$$\int_s D\cos\alpha\,dS=4\pi\sum_v q_c, \tag{3}$$

and the equations for the capacitance of condensers of various forms. As far as I am aware, no one questions the validity of this method of developing the subject of electrostatics. The two

[1] L. R. Wilberforce, Proc. Phys. Soc. London **45**, 82 (1933).
[2] L. Page, Phys. Rev. **44**, 112 (1933); L. R. Wilberforce, Proc. Phys. Soc. London **46**, 312 (1934); Page and Adams, Am. J. Phys. **3**, 51 (1935).

fundamental postulates find their justification in the agreement between experimental results and the derived relations.

Among the relations that can be derived from Eq. (1) is Eq. (2), but in its derivation one must make the qualifying assumption that the medium in which the free charges are immersed is isotropic and homogeneous throughout that part of space in which the electric field has an appreciable value. A large majority of textbooks develop electrostatics from this Eq. (2); very few of them mention the limitations to its validity. It is very convenient and easy to develop from Eq. (2) other equations involving q_c, but in doing so one imposes upon the derived equations the limitations of Eq. (2). These limitations are more drastic than seems to be generally realized. Very few situations actually occur in the laboratory to which the equation may be properly applied. It would be unimportant except for one circumstance. We may place the free charges on conductors that are immersed in an otherwise homogeneous, isotropic dielectric. Since the free charges automatically arrange themselves so as to make the field inside the conductor zero, the conditions are satisfied. This makes valid the development from Eq. (2) of the expression for the capacitance of a condenser having ideal guard rings.

One reason for the seemingly widespread failure to realize that Eq. (2) has only a limited range of validity may be the ease with which Eq. (3) can be derived using it as a premise. Knowing that Eq. (3) has general validity, one might wrongly infer from this that Eq. (2) is also valid under all conditions. This inference is incorrect because the derivation of Eq. (3) from Eq. (2) consists in evaluating a definite integral and the converse of this operation does not exist. In other words, Eq. (2) is a sufficient condition but not a necessary condition for Eq. (3).

With the two electrostatic equations in mind let us consider the corresponding situations in magnetostatics. We may write the equation

$$F=\frac{1}{\mu_0}\frac{pp'}{r^2}, \tag{4}$$

which is closely analogous to Eq. (1), and proceed with the development along the same lines. The analogy fails in two respects: (i) When we state the second fundamental postulate regarding the relation between field intensity and polarization we find that although it holds for paramagnetic and diamagnetic materials, it is not true for ferromagnetic substances; (ii) we find that the existence of the magnetic analog of free charge is generally doubted.

If there existed such a thing as a "free pole" p_c, one might expect that the equation

$$F=\frac{p_c p'_c}{\mu r^2} \tag{5}$$

would be valid provided p_c and p'_c were immersed in a homogeneous, isotropic, paramagnetic or diamagnetic medium which filled that part of space in which the magnetic field was appreciable. A careful reading of the 21 textbooks previously mentioned clearly shows that this is not what the authors had in mind when they introduced an equation having the form of Eq. (5). Rather the intention was to classify poles into two categories —poles due to the polarization of paramagnetic or diamagnetic materials, p_p, and those due to the polarization of ferromagnetic materials, p_f— and to write the equation

$$F=\frac{p_f p'_f}{\mu r^2}. \tag{6}$$

This equation is supposed to be applicable to the distributed poles of permanent magnets immersed in an otherwise homogeneous, isotropic, paramagnetic or diamagnetic medium of permeability μ. If the internal field of a permanent magnet were zero, as in the case of a charged conductor, the conditions would be satisfied. However, a permanent magnet that has an external field always has an internal field as well. The medium is not homogeneous throughout that part of space in which the magnetic field exists and therefore Eq. (6) is *incorrect*.

It is true that when a permanent magnet is immersed in a paramagnetic medium, the magnitude of the field at a point in its neighborhood is, in general, reduced. Under certain special conditions[3] involving the geometric shape of the magnet and its state of magnetization, the reduction of the field can be correctly expressed by the

[3] See reference 2.

introduction of a constant, say γ, into the denominator. It may easily be shown that in any such case $1<\gamma<\mu$. Similarly, if under the same conditions the medium is diamagnetic, the magnitude of the field at a point near the magnet is increased and $\mu<\gamma<1$. For no magnet of finite dimensions can the constant γ be equal to μ. Generally there is no such constant, the field changes direction as well as magnitude when μ is changed, and the changes in the magnitude of the field at various points in space are not proportional.

If μ is to be introduced into the denominator of the equation for Coulomb's law of magnetostatics, the symbols which appear in the numerator to represent the poles must be defined so as not to include the poles due to the magnetization of the medium. Two ways of doing this have been discussed. Equation (5) can be justified only if one is willing to admit the existence of "free poles." Equation (6) is incorrect because of the demagnetizing field inside magnets. It therefore appears that conditions analogous to those which validate Eq. (2) are not found in magnetostatics. Moreover, these conditions so limit the validity of Eq. (2) that Eq. (1) rather than Eq. (2) should be regarded as the fundamental law of electrostatics.

Electric Field Produced by Revolving Charges

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IF a random group of concentric circular orbits each having a charge $-Q$ revolving about an equal charge $+Q$, is considered, they are found to be surrounded by a negative field. The intensity of this field diminishes as the number of orbits increases and becomes zero when the number of orbits becomes infinite. In order to arrive at this result, it is necessary to consider first the electric field due to the rotation of a single charge in a plane.

A charge Q moving with a speed v in a circle of radius r (Fig. 1) produces at an external point O in the plane of the circle an electric field that is pulsating in character. Let d be the distance of the charge Q from point O at any instant and S the distance of O from the center of the circle. The impulse of the electric force on unit charge at O due to the charge Q during the interval dt is Qdt/d^2. The component of this impulse in the direction of S is $(Qdt/d^2)\cos\psi$, where ψ is the angle between d and S.

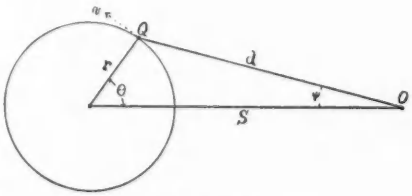

FIG. 1. Diagram of circular orbit.

If θ is the angle that r makes with S, then

$$\cos\psi=(S-r\cos\theta)/d,$$

and the impulse is expressed by

$$\frac{Qdt}{d^2}\frac{(S-r\cos\theta)}{d}=\frac{Q(S-r\cos\theta)dt}{(S^2+r^2-2Sr\cos\theta)^{\frac{3}{2}}},$$

since $d^2=S^2+r^2-2Sr\cos\theta$. The average force per unit charge, that is, the average field intensity at O in the direction of S, is therefore expressed by

$$E=\frac{Q}{T}\int_0^T\frac{(S-r\cos\theta)dt}{(S^2+r^2-2Sr\cos\theta)^{\frac{3}{2}}},$$

where T is the period of revolution of the charge Q.

Since $vdt=rd\theta$ and $T=2\pi r/v$, the foregoing expression for E becomes

$$E=\frac{Q}{\pi}\int_0^\pi\frac{(S-r\cos\theta)d\theta}{(S^2+r^2-2Sr\cos\theta)^{\frac{3}{2}}};\qquad(1)$$

its direction is that of S and hence is along a radius of the circular orbit.

By placing $a=r/S$, $b=2Sr/(S^2+r^2)$ and $c=\cos\theta$, we obtain from Eq. (1) the equation

$$E=\frac{Q}{\pi}\frac{S}{(S^2+r^2)^{\frac{3}{2}}}\int_0^\pi[(1-bc)^{-\frac{3}{2}}-ac(1-bc)^{-\frac{3}{2}}]d\theta.$$

This expression may be integrated by expanding into binomial series. The result obtained is

$$E=\frac{QS}{(S^2+r^2)^{\frac{1}{2}}}\{1.000000 +0.375000[2.50-(1+a^2)b^2] +0.410156[2.25-(1+a^2)b^4] +0.423000[2.1667-(1+a^2)b^6] +0.429600[2.125-(1+a^2)b^8] +0.433610[2.100-(1+a^2)b^{10}] +\cdots\}.$$

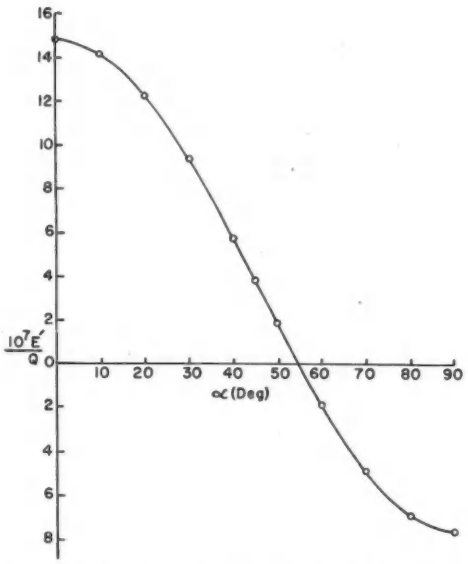

FIG. 2. Showing the variation in the electric field E as the angle α of the plane of the orbit is altered.

If $S=100$ and $r=10$, one obtains

$$E=\frac{Q}{10150.375}(+1.0+0.0219096000 +0.0007819973+0.0000294967 +0.0000011324+0.0000000438),$$

or $E=0.00010075673Q$.

If in this case there is a charge $-Q$ revolving in the circular orbit and a charge $+Q$ stationary at the center, then the average resultant field at O is

$$E=+\frac{Q}{(100)^2}-0.00010075673Q =-0.00000075673Q.$$

This indicates that if a negative charge revolves in a circular orbit about an equal positive charge

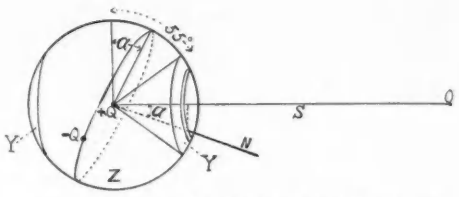

FIG. 3. Diagram showing the spherical zones.

at its center, the average resultant field at a point outside the orbit is negative. This field is alternating in character, but has a dominant negative amplitude.

Inspection of the terms in the foregoing expression for E shows that the field vanishes when S becomes infinite compared to r in the case of charges $-Q$ in the orbit and $+Q$ at the center.

If the plane of the circular orbit is at an angle $90°-\alpha$ with S, it can be shown that the field at O, because of a revolving charge Q in the circular orbit, is

$$E=\frac{Q}{\pi}\int_0^{\pi}\frac{(S-r\cos\theta\sin\alpha)d\theta}{(S^2+r^2-2Sr\cos\theta\sin\alpha)^{\frac{3}{2}}}, \quad (2)$$

where α may have any value from 0 to 180°. It is evident that Eq. (2) differs from Eq. (1) only in having $\sin\alpha$ as a multiplier of $\cos\theta$. When $\alpha=90°$, Eq. (2) reduces to Eq. (1).

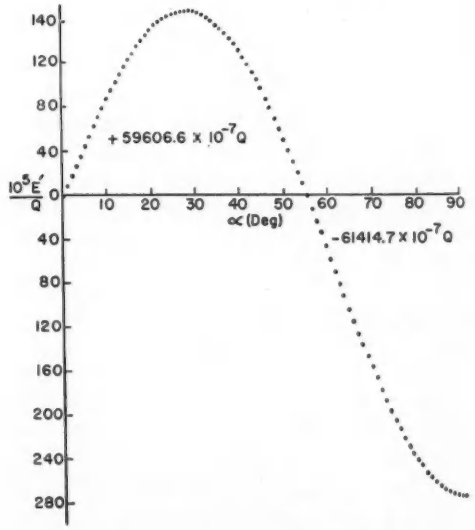

FIG. 4. Graph showing the total resultant contribution to the field by orbits at different values of the angle α, for $r=10$ and $S=100$.

Equation (2) may be integrated in the same manner as Eq. (1). The last factor in each of the terms in the series will be $(b \sin \alpha)^n$ instead of b^n. If, as before, $S=100$ and $r=10$, the series may be evaluated by multiplying the corresponding terms in the previous calculation by $(\sin \alpha)^n$.

If in this case also there is a charge $+Q$ at the center and an equal charge $-Q$ revolving in the orbit, the average resultant field will be

$$E' = \frac{Q}{(100)^2} - \frac{Q}{10150.375}[+1.0 + 0.0219096 \sin^2 \alpha + \cdots].$$

Figure 2 shows the values of E' obtained from this expression, for various values of α. The figure indicates that the value of α for which the field changes sign is 55°, which value apparently is independent of S.

It is of interest to note that in the case of any number of coaxial orbits, equally spaced in α, the resultant field at O is positive. For the distribution in α shown in Fig. 2, the resultant field is approximately the algebraic sum of the values given by the plotted points, namely, $+40.98 \times 10^{-7} Q$.

When, however, the distribution of the orbits is random, the situation is quite different. A group of orbits can be called random when each unit area on a sphere of radius r is pierced by the axis of *one* orbit. This conception gives a means of approach.

The transition value of α, namely, 55°, divides the surface of the sphere into a central zone Z and two end segments Y (see Fig. 3). All orbits having normals N that pierce the elementary unit areas of the end segments Y will contribute positive values to the field at O and all orbits having normals that pierce the elementary unit areas of the central zone Z will contribute negative values to the field at O.

For any given value of α all orbits having normals that pierce a circular ribbon of width $rd\alpha$ and whose plane is at right angles to S contribute equally to the field at O, and the total contribution for this value of α when $rd\alpha$ is unity is $E'2\pi r \sin \alpha$, or $E'360 \sin \alpha$ if the unit area is a square degree on the surface. When this latter expression is evaluated for angles α from 0° to 90° and a random distribution of orbits, each of which contains a charge $-Q$ revolving about a charge $+Q$ at its center, the values shown graphically in Fig. 4 are obtained. The ordinates of the points plotted in Fig. 4 are the contributions to the field at O of orbits having corresponding values of α. The value used for the contribution of each orbit was obtained from Fig. 2 enlarged.

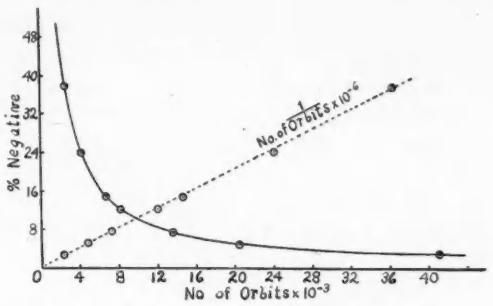

FIG. 5. Graph showing the percentage of excess of negative over positive contribution to field by different random groups of orbits.

The resultant field at O in this case, for 20626 orbits, is $E = -1808.0 \times 10^{-7} Q$. The total positive and negative contributions are given in Fig. 4. It is thus seen that in this case the total negative contribution exceeds the total positive contribution by 3 percent.

Computations similar to the foregoing made for unit areas of 1, 2, 3, 5, 6, 10, 15 degrees square, gave the results shown in Fig. 5, where the percentage excess of negative over positive contribution is plotted as a function of the number of orbits involved in each case.

It is thus seen that the electric field at any point O outside the orbits is negative until the number of orbits becomes infinite, when the resultant field vanishes. In that case the positive and negative contributions become proportional to the positive and negative areas under a curve similar to that of Fig. 4; as the number of orbits increases, the ratios both of the areas and of the positive and negative fields approach unity.

If the foregoing considerations hold in the case of atomic orbits, then each atom is surrounded by a negative electric field, since in this case the number of orbits is finite.

The writer wishes to express his appreciation to Professor W. E. Brooke for his aid in integrating Eq. (1) and also to Professors J. W. Buchta and E. L. Hill for helpful criticisms.

College Standards as Reflected in the Navy V-12 Program

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THE Navy V-12 College Program, which has been underway since July 1943, is no longer merely a plan for operation. It is now possible to talk about it on the basis of experience extending over many months.

PURPOSES

At a meeting in New York, in May 1943, of representatives from the colleges, Rear Admiral Jacobs summarized clearly and concisely the major purpose of the V-12 program as follows: "This is a college program. Its primary purpose is to give prospective naval officers the benefits of a college education in those areas most needed by the Navy."

It was clear that as the war progressed, college-trained men would no longer become available unless some steps were taken to have a continuing supply. Since such men formed the major source of supply for reserve officers, and to assure that the supply would continue to be available, the V-12 program was inaugurated.

Specifically, the program is designed to produce officers for the Navy: deck, supply corps and aviation, specialists in civil, mechanical, electrical, aeronautical engineering, aerology and physics, doctors, dentists, chaplains, and Marine Corps line and specialist officers. These groups supplement the usual supply of officers provided through the Naval Academy, the Naval ROTC units at 27 colleges and universities throughout the country and college graduates commissioned directly from civil life.

BASIC PRINCIPLES

In the early stages of planning and during operation of the program, the Navy has drawn heavily upon the experience of colleges and universities. A Naval Advisory Educational Council, consisting of outstanding college administrators, was appointed. This council worked closely with officers in the Navy in drawing up a set of general principles that would serve as guides in the development of the college program. Among these principles, a few stand out as exceedingly important.

(1) The resources of institutions of higher education were to be used throughout in the planning and in the actual operation of a program to train naval officers. The Navy would not attempt to provide new facilities to accomplish this purpose.

(2) In the development of curriculums, it was regarded as desirable that all naval officers should have included as part of their education a common core of subject matter. As the program actually developed, this core includes mathematics, English, history, physics, engineering drawing and naval organization. In addition, a physical fitness program is followed by all students in V-12 units. To teachers of college physics, the inclusion of that science in the common core is of course especially important. Through this requirement the Navy has placed a special burden upon them. The Navy is exceedingly gratified with the way in which they have responded, not only in providing this basic instruction but in training quickly some additional instructors so that the entire program could be carried forward smoothly and effectively.

(3) Another principle is concerned with the more advanced and specialized work. It was recognized early that the nature of this work would be determined by the specialties required in the Navy. The advanced programs therefore have included in them the subjects within the fields of specialization that have been found through experience to be most advisable.

(4) In preparing course descriptions for this program the Navy indicated that only a common minimum set of topics would be stipulated. Each institution is free to expand these minimum outlines as it deems advisable, on the basis of experience in carrying on similar instruction.

(5) Since the V-12 program was established as a war training program, it was definitely set up

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on an accelerated basis. Even so, however, students are permitted to take part in extra-curricula activities insofar as such activities do not interfere in their regular and required work.

These principles have been adhered to consistently. They have served as guides in the preparation of curriculums, bulletins and directives which have been sent to commanding officers in charge of units.

SELECTION OF COLLEGES

The process of selecting colleges to carry out this program was the work of a committee of nine with equal representation from the Army, Navy and War Manpower Commission. This procedure was established through an executive order of the President.

The first task of the committee was to canvass the facilities available at colleges and universities throughout the country. A questionary was prepared by the War Manpower Commission and sent to more than 1800 junior colleges, colleges and universities in the country. This gave each institution an opportunity to report the facilities it had available and that might be used for either an Army or a Navy program. The numbers of faculty members in various departments were reported, as well as classrooms, laboratory, housing and messing facilities. After sifting through this enormous amount of information, the committee approved 131 institutions which are being used by the Navy. In addition, it was agreed that 55 percent of the student capacity of each of the Class A Medical Schools would be made available for the Army program and 25 percent for the Navy program.

In any appraisal of the use being made of colleges and universities generally, it must be kept in mind that not only a competent faculty but physical facilities, such as dormitories and dining space, are essential. I think it can be said that most of the colleges and universities in the country having such facilities have been used in one way or another by the Army, the Navy or for some other war program.

SELECTION OF STUDENTS

In the selection of students for the Navy program, several sources of supply were drawn upon. First, there were the students who had enlisted in the Naval Reserve, either in the V-1, V-7 or V-5 programs. The V-5 program was established for aviation cadets; the V-7 included advanced students specializing in critical fields, such as engineering, physics and the like; the V-1 program was open to any student who had graduated from high school and was accepted by an accredited college. It is obvious from these brief descriptions that the degree of selection of students in them varied considerably.

Another group of students came directly from civil life. The first step in the screening process through which they passed was a qualifying examination given in approximately 16,000 high schools, colleges and universities. The Army joined with the Navy in giving these examinations. At the time the student took the examination, he was asked to indicate which branch of service he preferred. In general, the process of selection was rather rigid in order to insure a group of students who would be qualified to succeed with a difficult college program.

Another source of students was the group already in service; on the basis of the qualifications established, commanding officers throughout the Navy were given the opportunity to recommend men from within the service for the college program. A final group included the Marines who had either enlisted in the reserves or were already in service.

COLLEGE STANDARDS

The initial assignments to institutions of the students selected from these various sources were made in terms of the institution where the student was registered. That is, if a V-12 unit were established at the institution he attended, he would be permitted to remain, providing it offered the courses for the program in which he planned to specialize or for which he was selected. Students in other institutions where V-12 units were not located were moved to comparable institutions within the same area. An effort was made to reduce travel to a minimum. Altogether, students who were scattered in 1070 colleges and universities were on July 1, 1943 concentrated in the 131 institutions (aside from the medical, dental and theological schools) where the Navy

had made arrangements to carry on the college program.

With this major shift of students from institution to institution, it is clear that many problems of adjustment arose. Studies made over a period of time have indicated quite clearly that colleges and universities vary greatly in the standards to which they adhere. Some colleges found, for example, that students sent to them were not adequately prepared in mathematics to carry on the courses they provided in physics. For the most part, the initial estimates of attrition were high. This is not surprising, since these estimates were made during a period of adjustment of students to new institutional environments and adjustment of faculty to large groups of new students. The actual over-all attrition, however, at the end of the first term was less than 6 percent. In all probability, this represents normal attrition in these institutions.

Further evidence concerning the variation in standards of institutions was obtained through the use of comprehensive achievement examinations. Such examinations were set up to supplement other information available on the student so that he might be screened better for upper-level specialties. This examination covers in general the core courses offered during the first and second terms.

The first of these examinations was given in September 1943 to students in their second term of work. The results were comparable to those obtained by other extensive examination projects such as the Carnegie-Pennsylvania study, the North Central Association study which served as a basis for its accrediting procedures, the Co-operative Test Services program and other statewide and national studies.

All of these investigations show a wide variation among institutions. On the first comprehensive achievement examination given in the Navy program, the average scores for institutions ranged from the 17th to the 93rd percentile. In other words, for the institution that stood at the bottom of the list, one-half of the students who took the examination were in the lowest 17 percent of all students throughout the country; whereas for the institution that stood at the top of the list, one-half of the students were in the top 7 percent of all students throughout the country. The second examination was given in November 1943. On this the variation was even wider, extending from the 8th percentile for the institution at the bottom of the list to the 91st percentile for the institution at the top. Such information must be interpreted with considerable caution. One cannot infer that the quality of instruction alone is responsible for this wide variation. The quality of students is likewise important. The fact that must be recognized is that this variation exists and must be taken into account in any widespread planning such as that involved in the Navy program.

As already indicated, the results of these examinations are used exclusively for screening students to upper-level specialties. Other factors taken into consideration in the screening process were the college record, students' preferences for branch of service and curriculums, previous commitments, physical requirements and recommendations of the commanding officer. Thus an attempt was made here, as in other phases of the program, to utilize the best practices of colleges and universities generally in determining whether a student should be selected for one of the longer programs, completion of which would qualify him for one of the specialties of which the Navy has need. The first application of this screening process was completed at the beginning of the present year, and students were assigned accordingly on the opening of the next term, on March 1, 1944. The results will be studied with care in an effort to improve the procedures for successive terms.

After reviewing such facts, one might well raise the question as to what are the implications. Briefly, they may be summarized as follows.

(1) There exists in the Navy program, as in colleges and universities generally, a wide variation of standards. In this respect the Navy program is merely a reflection of the colleges and universities as they already exist. The Navy did not set out and does not now seek to reform higher education. It merely attempts to use the resources of the colleges and universities generally. Whether this variation in standards should exist is a problem not for the Navy but for the colleges and universities to solve.

(2) Although the aforementioned problem is

not the concern of the Navy to solve, the Navy is concerned with the qualifications of officers at the time they complete their program. Consequently, in the final commissioning of men for officers' ratings, the attainment of students must be taken into consideration. This is being done in several ways, particularly through the refinement of selection and screening procedures.

(3) It is clear from the information on variations among institutions that colleges need to define more clearly than they have in the past what they are trying to accomplish. This is a requirement that has been established as part of the accrediting procedures for the North Central Association and other accrediting agencies. In general, however, for colleges and universities throughout the country, this need has not been met.

THE FUTURE

It is difficult to make predictions in regard to the future of the V-12 program, as it is for any of the war programs. So much depends upon the course of the war.

In regard to selection procedures, refinements have already been made. Because of the early experience in the program, preparation in mathematics is now given greater emphasis both in the extent to which students have taken courses and in the qualifying examination.

For men coming in from the fleet, a minimum of two years in high school mathematics is required. Furthermore, the qualifying examination is now arranged so that one-half of it is devoted to a measurement of the student's proficiency in mathematics. Thus the experiences of the first term should not be repeated. In physics, for example, the students should have an adequate background of mathematics so that they will not experience the difficulty that some of the men did during the first term that the program was in operation.

In the field of physics, the program as set up established an over-all quota of 600 physics majors. Any adjustments in this number will again depend upon varying needs of the service.

There have been rumors that the program may end at any time. These are only rumors; the Navy has no plans for terminating the college program at a definite time. The program was set up to provide a continuing supply of officers for the duration. As to how long that will be, there have been many predictions, and with so many predictions, each of us can take his choice.

This program has been established through cooperative arrangements with the colleges. It will be maintained on the same basis. Its future depends not only upon the Navy but upon the colleges and universities generally. The physics departments have played and will continue to play an important part in this program. The Navy is deeply grateful to them for their aid and excellent cooperation and hopes that it may continue to call upon their good services.

An Important Notice Concerning Proposed Criterions for Self-Rating of Physics Departments

Every physicist who is interested in questions of the rating or accrediting of physics departments should write immediately to Professor Wheeler P. Davey, The Pennsylvania State College, State College, Pennsylvania, for a mimeographed copy of his paper, "Elements entering into a norm by which physics departments might rate themselves," which he presented by invitation at the recent Rochester meeting of the Association. Professor Davey has performed a fine service in working out a proposed rating system. As he has emphasized, many of his criterions necessarily are partly arbitrary in character; hence opinions concerning many of the details involved naturally will differ considerably. For instance, some physicists may feel that his plan favors the larger departments; others may think that more recognition should be given to a department's past record in producing good students; and so on. However, it should be possible to iron out most of these differences of opinion and to evolve a final scheme that all departments will find generally acceptable.

Those who have read the paper in mimeographed form should immediately send their criticisms or suggestions either to Professor Davey or to the Editor. Professor Davey's proposal will be printed in the December issue of the AMERICAN JOURNAL OF PHYSICS. All comments received but not taken into account in the printed version will either be published under "Notes and Discussion" in the JOURNAL or referred to a committee of the Association which almost surely will be appointed to consider the whole matter of rating and accrediting departments.—D. R.

A Modified Atwood Machine for Use by Elementary Students

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THE Atwood machine herein described was developed in the Department of Physics of Brooklyn College. Several members of the staff contributed valuable suggestions. The apparatus is the product of many attempts to develop a simple rugged Atwood machine for use in an elementary laboratory and in its present form has proved to be very satisfactory.

The apparatus (Fig. 1) consists of an iron casting which is rigidly screwed to a wooden board attached to the wall at a height of approximately 3 m from the floor (Fig. 2). The casting carries two ball bearing supported aluminum alloy pulleys, a brake and two bumpers.

Each bumper consists of an iron cylinder and a movable cast-iron plate. The cylinder is machined on the inside to fit the movable plate against which the masses are arrested. It is retained in the cylinder by means of a circular wire in a groove near the bottom of the cylinder. Loading is by means of a sponge-rubber cylinder which was cut from a large sheet by a circular die. The rubber was found to be more satisfactory than helical springs. The clearance between the movable plate and the inside of the cylinder is $\frac{1}{16}$ in.

Much of the success of the apparatus can be attributed to the unusual design of the masses, each of which consists of three steel cylinders as shown in Fig. 3. The top cylinders on each side are tied to the string and have masses of 212.5 and 250.0 gm, respectively. Each has eight vertical holes drilled into its top. Seven 5-gm masses and one 2.5-gm mass are placed in the holes of the 212.5-gm cylinder so that the combined mass of this cylinder and eight small masses is 250 gm. The total mass of the moving system is then 500 gm. By transferring the 2.5-gm mass to the other cylinder a mass difference of 5 gm is produced without changing the total mass of the system. This mass difference can be increased to 75 gm in 5-gm steps by transferring additional small masses. The total mass of the system can be increased in 500-gm steps from 500 to 2000 gm by attaching either or both of the lower steel cylinders to the top section as shown in Fig. 3.

Two 2-m bars supported to slide on each other are used to measure the distance traversed by the masses. Any timing device good to $\frac{1}{5}$ sec is suitable.

Since there is always a possibility of string failure, it is desirable to protect the students from falling weights. This is done by placing the narrow table, on which the students work, about 6 in. from the wall (Fig. 2).

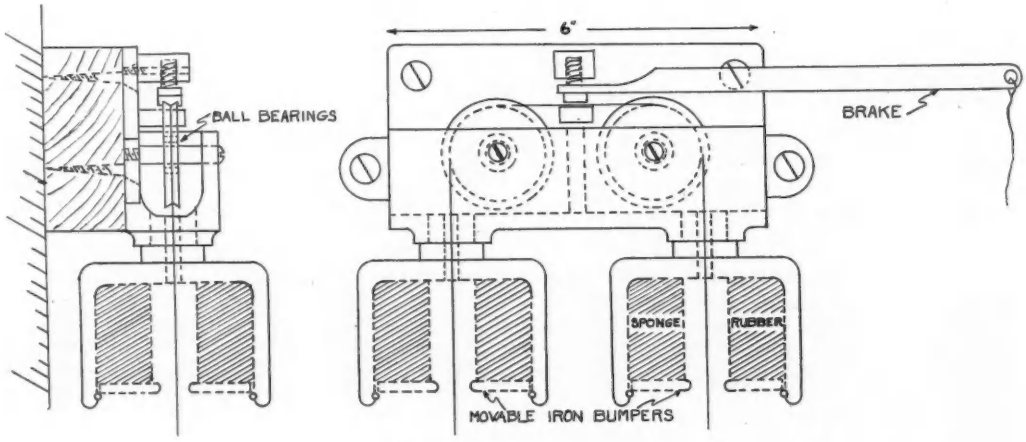

Fig. 1. Details of apparatus.

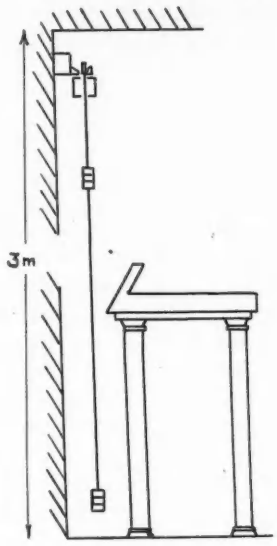

FIG. 2. Work table.

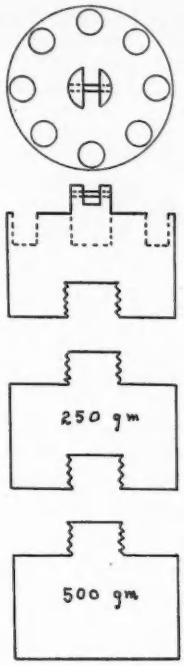

FIG. 3. Details of masses.

The students are asked to verify Newton's second law and to compute the acceleration due to gravity from the relation

$$g=\frac{(M_1+M_2)a}{(M_1-M_2)-R}, \qquad (1)$$

where M_1 and M_2 are the suspended masses, a is the acceleration computed from $a=2S/t^2$ and R

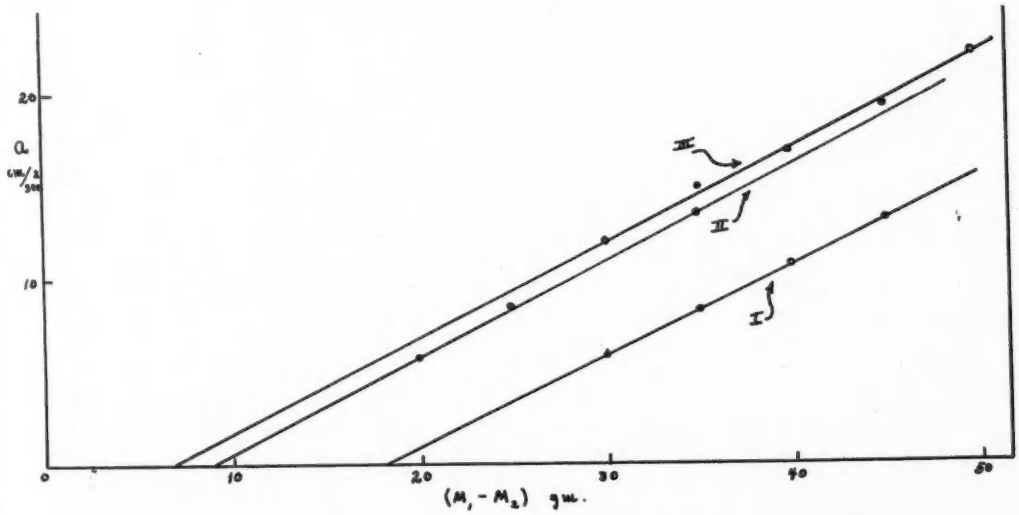

FIG. 4. Typical curves: I, thick string; II, thin string; III, thin steel wire.

TABLE I. Typical data for total mass of 2000 gm.

Support	M_1-M_2 (gm)	a (cm/sec²)	g (cm/sec²)
Thick string of diameter 0.060 in.	30	5.8	
	35	8.3	
	40	10.7	
	45	13.3	995
Thin string of diameter 0.025 in.	20	5.78	
	25	8.44	
	35	13.4	1010
Steel wire of diameter 0.016 in.	30	12.0	
	35	14.9	
	40	16.8	
	45	19.1	
	50	22.0	997

is the frictional force in grams obtained by extrapolation of a graph of M_1-M_2 *versus* a. The experimental points lie sufficiently close to a straight line so that g can be determined to about 2 percent. The error in the calculation of g introduced by neglecting the moment of inertia of the pulleys is

$$\Delta g=\frac{(M'+M'')a}{2[(M_1-M_2)-R]}, \tag{2}$$

where M' and M'' are the masses of the pulleys. This constitutes an error of approximately 0.6 percent when the total load is 2000 gm.

Typical data and curves for a total mass of 2000 gm are shown in Table I and Fig. 4. The data were taken with different types of strings. The use of small-diameter strings and steel wires to support the masses results in smaller frictional forces than when heavier strings are used, but the precision of the result is not improved and string failure increases. We therefore use heavy strings.

The Motives of Research

THE recent movement which exalts the material function of science and denies the right of pure science to an independent existence is a kind of philistinism which plays into the hands of those who dislike and distrust science and are only willing to concede its material services. One of the leaders of this movement tries to bolster up his case by rather childish misrepresentations of the motives of scientific research, suggesting that scientific curiosity is "not particularly noble," that the joy of new discovery, the delight in establishing some new fragment of objective truth, of contributing, in however small a degree, to a clearer insight into the order of Nature, are not the dominant impulses and feelings of the worker in pure science, that he is more often actuated by a desire to rise in the world, and that he will gladly turn his back on research to become an administrator. Such contentions are flatly contradicted by the experience of those actually engaged in research, who have the best opportunity of observing the dispositions and behavior of their fellow-workers. That many scientists do in fact turn to administrative work in their later years is perfectly true. The ardent curiosity of the true investigator and his capacity to continue the strenuous though delightful mental work involved in continuous research often weaken as the mind loses its early vigor, though some minds retain them throughout mature life. Nor can we assert that the attractions of an easier life and a larger income are negligible, especially to those who are conscious of the weakening of their earlier impulses. But the existence and effectiveness of these other motives in such cases do not affect the truth that the actual motives of pure research are highly characteristic and remarkably strong and pure. The evidence is plain to all who will look at the facts, unless they wish to believe the contrary.—A. G. TANSLEY, "The values of science to humanity," *Nature* **150**, 104–110 (1942).

Crova Disks for Projecting Slow Motion Longitudinal Waves

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A CROVA disk as described in current American apparatus catalogs is a cardboard disk with a series of printed circles used to demonstrate progressive longitudinal wave motion. The early models of these disks, as described by André Crova,[1] were made of glass and were designed for optical projection. Crova's models included disks for demonstrating both progressive and standing longitudinal waves, and also for demonstrating the reflection of single longitudinal pulses. Similar projection models for demonstrating both progressive and standing longitudinal waves have been listed in the catalog of E. Leybold's *Nachfolger* (ed. 3) without reference to them as Crova's disks. Disks for both standing and progressive waves have also been described in Weinhold's *Physikalische Demonstrationen* without any mention of Crova's name.

These simple devices as described by Crova have been found to provide extremely effective demonstrations of a subject which is otherwise difficult for students to visualize. A projection model for demonstrating progressive longitudinal waves, which was constructed by Professor J. B. Edwards, has been in constant use in our department for many years. Some time ago the writer constructed additional models for the projection of standing waves, unaware at that time that such models were already in existence. Because of the interest that has been shown by other physics teachers in these devices, and because the aforementioned descriptions are not readily available to all, we shall give a brief description of our models here.

Like those described by Crova, our devices for projecting longitudinal waves consist of rotating glass disks which have been made opaque except for a series of circles. On any one disk, these circles are almost but not quite concentric with the axis of rotation, and their radii increase uniformly from one circle to the next. Close behind the disk is placed a metal sheet with a narrow slit extending radially out from the fixed axis about which the disk rotates. When the device is placed in a projection lantern, a very short segment of each circle is illuminated through the radial slit. The segments of the circles which are illuminated therefore appear as a row of holes and may be projected on a screen as a row of light spots. Any spot in the row will oscillate longitudinally with approximately simple harmonic motion as the disk rotates, provided the center of the corresponding circle is displaced from the axis of rotation by an amount that is small compared to the radius of the circle. For any given spot, the amplitude of the oscillation will be equal to the displacement of the center of its circle from the axis, and the phase of its motion will be determined by the direction in which the center is displaced from the axis. Since the positions of the centers can be varied from one circle to the next, changes in amplitude and phase from one spot to the next can be made as needed to show either progressive or standing wave motion.

It is to be hoped that some apparatus manufacturer will make sets of these disks available in this country since the disks could easily be reproduced in quantity by a photographic method. For the benefit of those who might wish to make their own, some details of the construction of our disks will be given here. The disks now in use have a diameter of 9 in., and each has about 30 circles. The radius R_n of the nth circle on any one disk was made equal to $(1.0+0.1n)$ in., where $n=0, 1, 2, 3, \cdots$, so that the arcs of the circles are spaced about 0.1 in. apart on the average. The circular lines and the radial slit are each about 0.8 mm wide so that the spots to be projected are about 0.8 mm square. Since the row of spots to be illuminated is about 3 in. long, the apparatus can be used with a projection lantern of standard size.

The disks were prepared by spraying glass disks with black paint and cutting the circles through the paint after it was dry but before it became brittle. The compass used to cut the

[1] Ann. Chim. Phys. **12**, 288–308 (1867).

circles consisted of a small threaded rod with a pivot at one end and a piece of hacksaw blade sharpened like a cutting-off tool at the other end. To draw the circles, the disks were placed on a drawing board which was supported in a horizontal position above the bed of a lathe by clamping it between the chuck and tailstock. The 1-in. mounting hole through the center of the glass disk was placed over a hole in the drawing board so that a special hollow tool post could be extended up through both to receive the pivot end of the compass. The centers of the circles could thus be displaced from the center of the disk as desired, by using the micrometer screw of the cross-feed on the lathe. The angular coordinate of this displacement could be adjusted by rotating the disk on the drawing board against two centering stops at the circumference.

For a progressive wave of constant amplitude, all the spots must have the same amplitude and each spot must lag behind the preceding one by a constant phase angle. Hence on the disk made to show a progressive wave, the centers of all the circles were displaced by 0.17 in. from the axis of rotation. The backward shift in phase was secured by turning the disk 18° each time a new circle was drawn, so that the distance between adjacent spots was $1/20\ \lambda$.

For standing waves the amplitude of oscillation must vary from spot to spot according to a sine function, while the phase remains constant except at the nodes, where a reversal in phase occurs. Thus disks for standing waves were made with the centers of the successive circles all lying on a straight line through the axis. An amplitude of 0.25 in. was used for the spots at the loops of the standing waves. Accordingly, the displacement y of the center of any circle was computed by the formula

$$y = (0.25 \text{ in.}) \sin [2\pi(n/k) + \alpha],$$

where k is the number of spots desired per unit wave-length λ and n is equal to 0, 1, 2, $\cdots$ for the first, second, third, $\cdots$ circles. The constant α is equal to zero if the spot from the first circle is to be a node, and is equal to 90° if it is to be a loop. Since y becomes negative when the sine becomes negative, it is not necessary to rotate the plate 180° to reverse the phase at the nodal points. One standing-wave disk was constructed in this way with $\alpha = 0$, $k = 36$, and the total number of circles equal to 28; this gave a standing wave $\frac{3}{4}\lambda$ long with a node at the inner end, a loop at the outer end, and an intermediate node at the 19th spot, where n is 18. Another disk was constructed with α equal to 0, k equal to 28, and with a total of 29 circles; this gave a standing wave of length λ with a node at each end and an intermediate node at the 15th spot, where n is 14. It will be obvious from these examples how other disks could be made with any desired pattern of nodes and loops.

Each disk is permanently mounted on an axle consisting of a small brass rod which may be slipped into a simple sleeve bearing. The moment of inertia of such a disk is large enough and the friction of the bearing small enough so that the disk can be turned satisfactorily by hand at any desired speed.

Formation of a Division of Physics in SPEE

AT the Cincinnati meeting of the Society for the Promotion of Engineering Education a petition was presented to the Council asking for the formation of a Division of Physics to replace the former committee. Such a Division would have a continuing existence and plans could be made for future activities. The petition was granted and an organization meeting was held at which the following officers were elected: *Chairman*, J. G. Potter, Bell Telephone Laboratories; *Secretary*, C. E. Bennett, University of Maine; *Executive Committee*, in addition to the chairman and secretary, H. L. Dodge, Norwich University; J. H. Howey, Georgia Institute of Technology; J. W. Woodrow, Iowa State College. The following other members of the SPEE were also present at the organization meeting: W. H. Billhartz, Franklin College; A. Hazeltine; L. M. Heil, Cooper Union; F. L. Partlo, Michigan College of Mining and Technology.—J. D. E.

A Simple Pendulum Energy Experiment

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SEVERAL elementary experiments have been described[1] in which use is made of a spark or photographic record of the path of a simple pendulum in the study of some aspect of its motion. The conservation of energy experiment described by Collins[2] has suggested that a similar experiment might be designed to test the equation

$$\tfrac{1}{2}mv_0^2 = mgh \qquad (1)$$

for a simple pendulum, by making direct measurements of the maximum velocity v_0 and the elevation of the bob. When a rough form of such an apparatus was tried out, the results were sufficiently good that it seemed worth while to make some refinements in the apparatus and set it up in more permanent form.

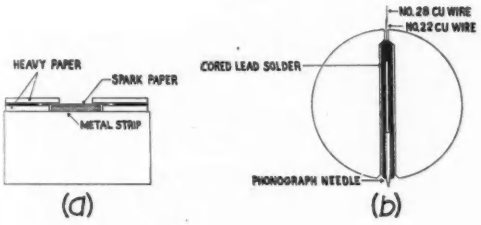

FIG. 1. (*a*) Cross section of track. (*b*) Pendulum bob and sparking point.

APPARATUS

The set-up differs only in detail from that described by Constantinides[3] for the study of simple harmonic motion. A suitable track was made by sawing arcs of 100-cm radius from three $\frac{3}{4}$-in. boards; these were screwed together and the track scraped smooth. A strip of $\frac{5}{8}$-in. metal box-binding material, fastened along the center of the track with Glyptal paint, furnished a satisfactory electrode. The track so formed was about 60 cm long and 6 cm wide, with a radius of curvature of approximately 100 cm. Strips of heavy paper were glued along the track, as indicated in Fig. 1(*a*), to hold the spark paper in contact with the metal strip. The usual 2-in. spark paper was cut in two, and a strip was slid under the paper holder. The pendulum bob was a 1-in. drilled steel ball with a phonograph needle attached as shown in Fig. 1(*b*). A small electromagnet with a pointed core was used for holding the bob when it was drawn aside, and releasing it when desired.

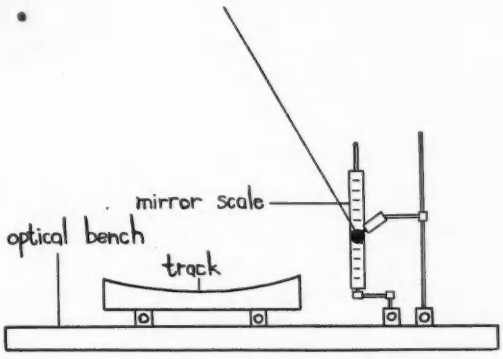

FIG. 2. General schematic of apparatus.

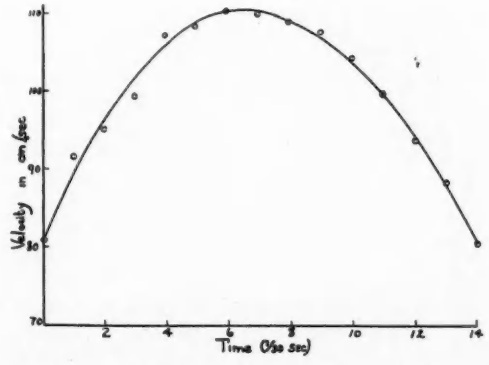

FIG. 3. Graph for determining maximum velocity v_0 of pendulum bob.

[1] K. D. Larsen, Am. J. Phys. **5**, 130 (1937); I. Walerstein, Am. J. Phys. **7**, 190 (1937); P. A. Constantinides, Am. J. Phys. **7**, 417 (1939).
[2] E. Collins, Am. J. Phys. **12**, 44 (1944).
[3] See reference 1.

TABLE I. Distance traveled and velocity for successive time intervals of 1/30 sec and height of fall of 6.07 cm.

Distance (cm)	Velocity v_0 (cm/sec)
2.69	80.7
3.05	91.5
3.17	95.1
3.31	99.3
3.58	107.4
3.61	108.3
3.68	110.4
3.67	110.1
3.64	109.2
3.63	108.9
3.48	104.4
3.33	99.9
3.13	93.9
2.95	88.5
2.68	80.4

All of the apparatus except the pendulum, which was supported on a heavy support rod and base, was mounted on an optical bench (Fig. 2). The track was held on two carriages, the hinged tops being folded back. A wooden stop, which just fitted the framework of one of the carriages, was nailed to the under side of the track to insure getting the track back in the same position after it was once adjusted. A third carriage supported the electromagnet and a fourth a vertical mirror scale. The scale was offset just enough to allow the bob to clear, and since the bench was leveled, the scale could be moved to measure h for any displacement. Timing marks on the recording paper were obtained with the aid of a synchronous spark timer giving 120 sparks per sec. However, a few trials were made with a vibrating fork timer, with results that were satisfactory but somewhat less accurate than those obtained with the synchronous timer. Best traces were obtained with the sparking point as close to the paper as practicable; most traces were run with the sparking point about 1 mm from the paper.

TABLE II. Square of maximum velocity, v_0, of pendulum bob for various heights of fall.

h (cm)	v_0 at track, from graph (cm/sec)	v_0 at bob (cm/sec)	v_0^2 (cm^2/sec^2)
3.96	90.0	88.6	7850
6.07	110.7	109.0	11880
8.06	128.4	126.3	15950
10.08	142.5	140.2	19660
12.06	156.3	153.8	23670
14.07	168.6	165.7	27460
16.18	181.2	178.3	31790

PROCEDURE

The maximum velocity v_0 is found as follows. A meter stick is laid along the record so that all marks are visible near the scale; starting with a chosen reference point near one end of the trace, readings are recorded for every fourth spark. The successive differences between these readings give the distances traveled in four spark intervals, or 1/30 sec. The distance-time quotient represents the velocity at the track (Table I).

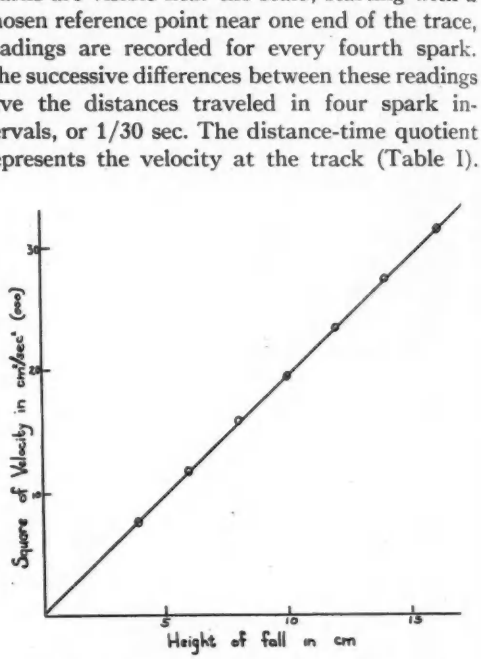

FIG. 4. Graph of v_0^2 as a function of h, the elevation of the bob.

The velocity at the center of gravity of the bob is easily found from a knowledge of the length of the pendulum and the distance from the pendulum clamp to the arc.

On plotting these velocities against successive time intervals, the student obtains a graph like that in Fig. 3, in which the highest point represents the maximum velocity.[4] This value may then be compared to that obtained from the equation $v_0 = (2gh)^{\frac{1}{2}}$.

It is also instructive to calculate v_0 from the equations of simple harmonic motion, by measuring the period of the pendulum for small swings and taking the amplitude measured along

[4] It should perhaps be noted that it is preferable to favor the higher points in drawing the curve.

the arc. The amplitude is easily determined for cases where the bob can be started over the paper, by marking the starting point and the equilibrium position by a short series of sparks.

Perhaps a better method of presenting the experiment, if time permits, is to make a series of runs for several different values of h. To determine the maximum velocity in each case it is only necessary to plot a few points in the neighborhood of the maximum. The student may then plot a graph of v_0^2 *versus* h and obtain a more general verification of Eq. (1). From the slope of the graph he may also compute a value of g. The same purpose may be accomplished by plotting s_0^2 *versus* h.

Seven values of v_0^2 as a function of h are listed in Table II and plotted in Fig. 4. The difference between the measured value and the computed value of v_0 for any one of these runs is less than 1 percent. The value of g determined from the slope of the line is 979 cm/sec^2. If this method of presentation is used it may be found convenient to have most of the spark records prepared in advance of the laboratory period, and to run one or two as a demonstration. Each laboratory group may then measure one record completely and pass the data along to the next group, and so on, so that in the end each student has all of the necessary data for plotting the graph.

In computing the theoretical value of v_0 it is possible to take into account the loss in energy due to friction, by measuring the decrease in amplitude during one complete swing and assuming one-fourth of this for the decrease during the first quarter-swing. The decrease is easily measured by first marking the initial position of the bob, then closing the spark timer key on the return swing. With the present apparatus the resulting correction to h was found to be of the same order of magnitude as the error in measuring h, and was therefore neglected.

A Laboratory Method for the Study of Projectile Motion

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AN apparatus for verifying the laws of projectile motion has been developed; it consists of a spring gun and trigger mechanism so mounted as to permit varying angles of elevation and rotation in azimuth. The range can be adjusted easily by changing springs or by a screw head permitting varying compression of the spring. Velocities from 800 to 1200 cm/sec are obtained with ranges up to 20 m.

A laboratory model of this apparatus permits a quantitative study of the motion of the projectile; the length of the latus rectum and the coordinates of the focus and of the vertex of the parabolic path of the projectile can be determined and compared with those predicted by theory.

THEORY OF PROJECTILE MOTION IN VACUUM

The equation of motion of the projectile is

$$d^2y/dt^2 = -g, \qquad (1)$$

where y is the vertical distance traversed by the projectile and g is the acceleration due to gravity. Integrating Eq. (1), one obtains

$$dy/dt = -gt + c_1$$

and

$$y = -\tfrac{1}{2}gt^2 + c_1t + c_2.$$

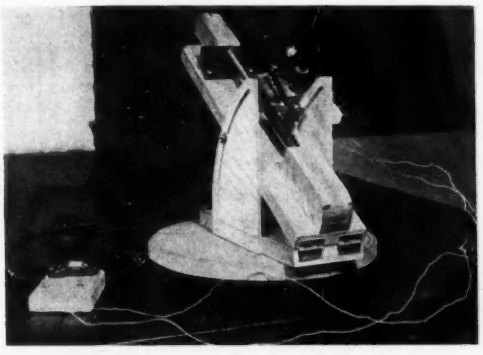

FIG. 1. Photograph of the apparatus.

TABLE I. Experimental and theoretical range and height for various angles of elevation.

θ	Range, X (cm) Theor.	Obs.	Difference (percent)	Maximum ordinate, Y (cm) Theor.	Obs.	Difference (percent)
17° 53′	650	587	9.7	53	40	24.5
22° 49′	796	750	5.8	84	70	16.6
27° 41′	917	882	3.8	120	103	14.1
33° 20′	1022	1004	1.8	167	155	17.2
39° 49′	1094	1074	1.8	228	205	10.1
43° 42′	1111	1091	1.8	265	245	7.5
48° 44′	1102	1086	1.5	314	296	5.7
51° 25′	1085	1066	1.8	340	324	4.7
56° 20′	1027	1005	2.1	385	370	3.9
60°	964	930	4.8	417	397	4.8
65°	852	815	4.3	457	439	3.9

If $y=0$ and $dy/dt=v\sin\theta$ when $t=0$, then $c_1=v\sin\theta$ and $c_2=0$, where v is the muzzle velocity and θ is the angle of elevation. Thus,

$$y=vt\sin\theta-\tfrac{1}{2}gt^2. \tag{2}$$

At the maximum ordinate Y, $dy/dt=0$; then

$$\tfrac{1}{2}T=(v/g)\sin\theta, \tag{3}$$

where T is the total time of flight for horizontal range X, and

$$Y=\tfrac{1}{2}(v^2/g)\sin^2\theta. \tag{4}$$

Hence,

$$X=(v\cos\theta)T=(v^2/g)\sin 2\theta. \tag{5}$$

Eliminating v^2 from Eqs. (4) and (5), one obtains $Y=\frac{1}{4}X\tan\theta$, or $X=4Y\cot\theta$. Since $x=vt\cos\theta$, one obtains from Eq. (2) the equation of the trajectory referred to rectangular axes with origin at the muzzle of the gun. This equation,

$$y=x\tan\theta-\tfrac{1}{2}gx^2/v^2\cos^2\theta,$$

is the equation of a parabola with latus rectum of length $2(v^2/g)\cos^2\theta$; the coordinates of its vertex are $[(v^2/g)\sin\theta\cos\theta,\ (v^2/2g)\sin^2\theta]$ and those of its focus are $[(v^2/g)\sin\theta\cos\theta,\ (v^2/2g)(\sin^2\theta-\cos^2\theta)]$. From Eq. (5) one may determine the velocity of the projectile or the range for a known angle of elevation.

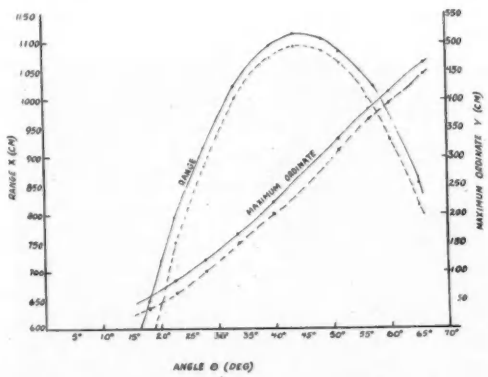

FIG. 2. Theoretical and observed ranges and maximum ordinates for various angles of elevation. – – – –, theoretical; –·–·, observed.

THE APPARATUS

The base of the instrument is a hardwood disk of diameter 50 cm, graduated in degrees. An inclined plane, 40 cm long and 15 cm wide, is hinged to a horizontal piece of the same dimensions attached by a bolt to the center of the base. The inclined plane used in elevating the gun and mechanism may be held in position at a given angle by bolting to fixed scales. The angular elevation is read from a printed Pyralin surface glued to the fixed pieces of wood. (See Fig. 1.)

The gun consists of a rod especially made for the purpose from a solid piece of $\frac{3}{4}$-in. rolled steel. The rod is moved forward by a compressed spring. A trigger mechanism holds the rod in position for release when the spring is compressed. The trigger is released by an electromagnet.

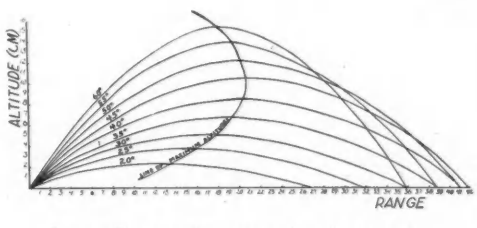

FIG. 3. Trajectories obtained with the device.

The projectile, a steel ball, is placed in a short metal-lined cylinder which is constructed as a part of the elevating plane in such a position that, on release of the trigger, the rod makes contact with the projectile at a point through the bottom of the cylinder where maximum force is attained. Balls 1.5 cm in diameter made of steel, copper, aluminum, brass and lead have been used, steel proving to be most effective. The heavier balls are better.

DISCUSSION OF RESULTS

The muzzle velocity v is determined by having several observers take about 100 recordings of the time of flight T for a given range X and a

TABLE II. Computed values from artillery trainer. Muzzle velocity v, 1113 cm/sec, or 36.6 ft/sec.

Angular elevation, θ (rad)	(deg)	Range, X (cm)	(ft)	Maximum ordinate, Y (cm)	(ft)	Muzzle velocity Vertical, v_x (ft/sec)	(cm/sec)	Horizontal, v_y (ft/sec)	(cm/sec)	Vertical area covered (ft²)	Time of flight, T (sec)
$\pi/9$	20	815	26.75	74.1	2.44	13.1	399	34.2	1040	44.1	0.78
$5\pi/36$	25	970	31.90	113.0	3.71	15.5	470	33.2	1006	85.0	0.97
$\pi/6$	30	1099	36.00	157.0	5.16	18.3	556	30.5	962	131.5	1.14
$7\pi/36$	35	1189	39.00	208.0	6.84	21.0	639	29.9	910	149.5	1.31
$2\pi/9$	40	1246	41.00	261.0	8.59	23.5	715	27.9	850	245.0	1.46
$\pi/4$	45	1263	41.60	316.0	10.40	25.8	786	25.8	786	336.0	1.61
$5\pi/18$	50	1246	41.00	371.0	12.21	27.9	850	23.5	715	409.9	1.74
$11\pi/36$	55	1189	39.00	424.0	13.92	29.9	910	21.0	638	480.7	1.87
$\pi/3$	60	1099	36.00	474.0	15.55	30.5	962	18.3	556	542.0	1.96

given angle of elevation θ (say 30°). From Eq. (3) and the known values for T, θ and g, the muzzle velocity can be computed. This was found to be, for a particular set of conditions, 10.44 m/sec. When the projectile was shot horizontally, a velocity of 11.00 m/sec was obtained. Similarly, firing the projectile into a ballistic pendulum yielded the comparable value 10.25 m/sec.

Using the value of v obtained from the first method, one obtains the computed values of the range and maximum ordinates given in Table I; this table also includes, for comparison, the observed ranges and heights. Figure 2 provides a comparison of the theoretical and experimental values. The results are in good enough general agreement for the demonstration of the principles in ordinary laboratory work. The percentage errors in range appear to be larger for smaller angles.

OTHER OBSERVATIONS

Table II gives the complete history of the motion of the projectile for one set of observations with the apparatus. In this case the curves for the trajectories were plotted accurately and the areas under these curves checked by planimeter integration. The maximum ordinates were determined in this case by placing a vertical stand of adjustable height in the path of the projectile. On this stand were placed wooden prongs which moved individually when struck by the projectile. The vertical distance to the prong that moved could be measured accurately; and by adjusting the stand to the proper position along the horizontal, the position of the maximum ordinate was determined.

Figure 3 shows the trajectories of the projectile and the positions and values of the maximum ordinates.

Postwar Training of Physicists for Industry

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ANY attempt at formulating ideas on the postwar training of physicists must be preceded by a consideration of the vital problem of the social, economic and political structure of the world in the period that will follow immediately after the cessation of war. Much has already been published on this topic by so-called authorities and experts. The predictions range all the way from extreme pessimism to unrestrained optimism. Since the writer is neither an expert nor a crystal-gazer, any prognostications that he might offer would no doubt seem highly speculative. However, we may perhaps be able to deduce from the experience of the past few years, and from a consideration of present trends, some conclusions that should be fairly reasonable. These conclusions may be listed as follows:

(1) The American way of life will *not* be modified to any radical extent. We do not face danger of either a bolshevik upheaval or a re-

actionary reversion to the so-called "normalcy" of the twenties. The tendency towards increased socialization, towards government control of industry and commerce, will continue. The policy of laissez-faire which was held sacrosanct until the past decade is definitely discredited. There will be decreased profits for industrial organizations, especially the large ones, and a leveling of incomes for individuals. On the other hand, there will be greater security for labor, and employment will be more general than in any period before the war. There has been a growing feeling that we are individually responsible for one another and that opportunities for work for those who desire it must not be denied to anyone through failure of our economic system.

During the past three years, Stuart Chase has published at least three volumes, under the sponsorship of the Twentieth Century Fund, which deal with both prewar and probable postwar economic and social conditions. I believe that one of these volumes, entitled *Goals for America*, ought to be of special interest to scientists. The picture he draws of the new industrial resources—industrial experience and trained personnel—which we will have available in the postwar period, is extremely stimulating. He draws attention to the "vast budget of postponed wants to be filled" within our own country and of the equally urgent demands from abroad for assistance in rebuilding a shattered world. Chase says:[1]

> Americans will find themselves with plenty of tasks to do. They will have the trained manpower, the plant, the energy, the raw materials, the machine tools, to do it with. Failing a very long war which erodes away much of the plant, the outlook in physical terms is encouraging. There should be no material reason for not carrying American civilization forward, as well as supplying many victims of the war beyond our borders. We shall be physically equipped to do both.

In this world which will arise in the next few years, old economic doctrines held to be well-nigh infallible, from the days of Adam Smith, will have to be thrown overboard, even as the Ptolemaic theory was discarded by the followers of Copernicus over four centuries ago.

Men who have faced death and seen their comrades fall in a slaughter brought on by personal ambitions, by fantastic ideologies and by the stupidity of men in high places—such men will not be satisfied with ancient shibboleths and dogmas which have been found to be not only fallacious but even deterrents of progress. To believe that the world can retrace the steps of the period 1918–1930 seems to me inconceivable. To attempt such a course is to court disaster such as the human race has not experienced even in this greatest of wars. For their own self-preservation men and nations will have to adopt radically new ideas and new ways of doing things.

(2) It is a familiar fact that, because of the war effort, civilian demands for even essential products have been severely curtailed. There are huge shortages in all kinds of goods and facilities, ranging from housing to automobiles, from flatirons to needles. Moreover, outside of the established industries which in peacetime cater to the demands of our American standard of living, there will be new developments which owe their origin to the war. The last war furnished the impetus for radio communication: The present war has seen the development of ultra-high frequency technics, of radar and innumerable applications of electronic devices. Television will become the complementary industry to radio.

Air-conditioning of homes and the use of electric appliances for the elimination of household drudgery will become more and more general. In the fields of plastics and light metals, there are equally interesting developments and applications which will be realized more fully when conditions permit.

The airplane industry has made marvelous strides during the war. As a vehicle of transportation for both commercial and pleasure purposes, the airplane will become well-nigh as popular as the automobile and truck. Nor should we omit the vast progress in medicine and surgery, which is bound to affect profoundly our health and length of life, as well as our social conditions.

To those readers who are under 40 years of age, this admittedly optimistic view of the future may seem far-fetched. But let us compare 1944 with 1914, with respect to standards of living. In 1914, the tungsten vacuum lamp was the best source of illumination available. Ductile

[1] Chase, *Goals for America*, p. 4.

tungsten filaments were being introduced to replace the brittle filament used in the earlier lamps. The cost of electric power to the householder was prohibitively high. In 1944, even the gas-filled tungsten lamp, operating at twice the efficiency of the old vacuum lamp, is gradually receding before the fluorescent lamp, which operates at an efficiency of four to six times that of the gas-filled type. Moreover, the cost of electric power has also decreased considerably.

In 1914 electric refrigerators, washing machines, dishwashers and similar devices were available only to the wealthiest. The vacuum cleaner was an imperfect device. Radio broadcasting was not even dreamt of. Motion pictures were still in the nickelodeon stage and the possibility of sound films, even if someone did indulge in such a radical vision, seemed quite remote.

Only a relatively small number of our population owned automobiles, and the best of 1914 was certainly vastly inferior to our cheapest machine of 1940. The airplane was still a mysterious curiosity, to be handled only by the rare individual.

Is it not highly probable that the industrial progress made during this period which has just ended will be duplicated and even exceeded during the next 30-year period?

(3) The third conclusion which we can infer from experience both before and during the present war is that modern industrial progress can be achieved only through the efforts of research and developmental scientists and engineers. From recent experience, it is evident that the demands made upon these men are constantly becoming more exacting and difficult, as well as more numerous. In 1914, the physicist had available as tools for research the electrometer, the spectroscope, a few electrical measuring instruments and, in the then recently discovered field of x-rays, a gas-filled tube. In 1944 we have not only a 2,000,000-v x-ray tube, but also a 100,000,000-v electron accelerator and the cyclotron.

In addition, we have the electron microscope, electron diffraction, supersonics, vacuum-tube circuits that serve literally a thousand purposes, cathode-ray oscilloscopes, high frequency and ultra-high frequency oscillators and many other devices. Problems that could not be solved in 1914—in fact, problems that were not even formulated at that time—are now handled in almost routine manner by many devices and methods developed by the physicists during the past 30 years.

Many, if not most, of these devices and methods have, during the past few years, found important applications in industry, and the war has catalysed these applications to a vast extent.

Moreover, industry is now learning that physicists are capable of contributing to, and initiating, fairly complex engineering developments. The design and operation of a cyclotron require a technical skill such as compares very well with that required to design and operate a high voltage generating equipment. The physicist has indeed demonstrated by his activities in the war effort that he is not a theoretical recluse intent only on some abstract objective, that, in fact, he can become, under proper conditions, just as practical and just as hard-boiled as any industrial engineer.

In view of these considerations, it would seem reasonable to conclude that in the postwar world there will be a much greater demand for physicists in industry than in the past. As Dr. A. W. Hull has recently pointed out,[2] the value of research in industry has justified itself so well that it is no longer regarded as a luxury to be indulged in by large corporations, but rather as a prime need and stimulation for further progress. The problem which we have before us today is the very important one of determining the nature of the training which can accomplish most in fitting physicists to take their part in this highly technical age that will follow the war.

I am, of course, well aware of the fact that in a number of institutions there have been organized courses in industrial or engineering physics. The primary objective of such a course should be that of making the physicist more practical, more versatile, and better fitted to cooperate with engineers in various fields of technical work. However, there is always the danger that the engineering aspects will be overemphasized at the expense of the fundamental training in physics.

[2] Hull, Am. J. Phys. **12**, 62 (1944).

There is a basic difference between the mental attitude of the physicist and that of the engineer. The physicist is trained to preserve a critical, searching attitude towards nature. He is continually searching for basic facts, for fundamental laws and for general principles. The engineer seeks to utilize these observations and conclusions for the accomplishment of technical objectives.

Naturally, there is considerable overlapping between physicists and engineers, and at no time in the history of science has such overlapping been more striking than at the present time. Indeed, the highest attributes of both physicists and engineers may often be present in the same individual. But on the whole, there is a difference between the motivating philosophies of the physicists and engineers, and a training in physics should preserve this difference.

As the result of my own experience of over 30 years in an industrial laboratory, I feel convinced that the most essential feature of any course of training in physics should be emphasis on fundamental principles and concepts, rather than an attempt to cover comprehensively the different branches of the subject. If I were asked to coin a slogan for the guidance of teachers, I would say, "Less and better."

Classical physics is still the basis of this branch of science; a thorough, working knowledge of mechanics, electromagnetic theory, geometrical optics and acoustics is absolutely essential for the physicist in industry. Illustrations should be taken, as much as possible, from present-day industrial applications; but the principal objective of training should be that of familiarizing the student to the maximum extent with the physical significance of the basic concepts and laws.

With regard to the training in mathematics, I shall quote from my article in 1937.[3]

> Mathematics is usually taught by pure mathematicians who are interested in refinement of logic and in their subject as an end in itself. But for the physicist, mathematics is a tool, and a very useful one at that. Hence, the mathematics ought to emphasize the practical aspects. Let the students be taught a broader but less intensive training in a number of mathematical fields. When the need arises, the capable student can fill in gaps in his knowledge by going to the proper sources. The main thing is that he should know the type of problem which can be treated by a given mathematical technic, and he need not waste his time learning all the side-paths and pitfalls which delight the mathematician. Would I advocate therefore that mathematics be taught by a mathematical physicist? By no means. Let the mathematicians teach the fundamental courses as far as differential equations and Fourier's series, teaching them along broad lines, and let a physicist show the students how these mathematical methods fit into the solution of problems which are of practical interest.
>
> Fortunately there are now available a number of textbooks in which there are excellent illustrations of the function of mathematical analysis in the interpretation of physical phenomena. There is a great deal that is instructive and stimulating in the study of the fundamental generalizations which have been derived by men who were "likest gods." There will always be a small percentage of students who will enjoy this intimacy with the great mathematical intellects of the past and present. But for the large percentage of students, who are so-called practically minded, the teaching of mathematical physics is made altogether too large a part of the course.

In the field of modern physics it is, of course, necessary that the student should be familiar with those observations which have been designated as quantum phenomena, such as the photoelectric effect, absorption and emission of radiation in the different spectral regions, the idea of energy levels, and so forth. However, it is very questionable whether much effort should be spent on quantum mechanics, that is, upon the mathematical technics which have been developed by Schrödinger, Heisenberg, Dirac and others. The physicist training for industry will find it much more advantageous to devote the time and effort that would be required for this purpose to more difficult theoretical topics in classical physics.

The same considerations should apply to the study of nuclear phenomena. A general knowledge of the observations and conclusions is all that would seem essential.

Outside of the purely fundamental courses, the physicist in industry should also possess at least an elementary training in direct- and alternating-current electrical engineering, which covers not only a knowledge of principles of operation of common electric apparatus, but also a familiarity with their actual operation as acquired in the laboratory.

[3] Dushman, J. App. Phys. **8**, 59 (1937).

The experience during recent years, and especially that connected with our war effort, has focused a great deal of attention on the almost infinite number of uses for electronic devices, both of the vacuum and gas-filled type. The utilization of these devices in industrial operations of different kinds is increasing daily, both for control of industrial processes and for research and developmental work. The applications in radio communication, television and radar constitute also an ever-expanding field. The lecture course in this field should be supplemented by laboratory work in which the student will acquire a measure of familiarity with many of the actual commercial types of tubes and with the circuits in which they may be used.

The great danger exists that a course in electronics may be made more extensive than is really necessary, thus sacrificing other essential subjects. One important fact likely to be overlooked is that the graduate will have to acquire most of his education in a field such as electronics through postgraduate work in industry. The college can give the student a start in the subject; but special technical details, procedures, and so forth will have to be learned as the need for the application arises. Indeed, in many of the larger industrial organizations it is customary to have special postgraduate courses in which the new technically trained employee is informed regarding the most recent developments in each of a number of fields.

An extensive experience in the laboratory should be incorporated in the course required of a physicist who looks forward to industrial employment. As far as possible, the experiments should not be of the type where the apparatus is already set up and all the student has to do is to throw a switch and read instruments. The student should assemble the apparatus himself. He should learn how to set up a spectroscope and to take spectrograms; to use a cathode-ray tube; some of the essential details of vacuum technics, and how to set up some of the simpler circuits involving the application of electronic devices. He should have some familiarity with the operation of x-ray tubes, and the procedure for obtaining x-ray diffraction patterns of crystals. In short, the industrial physicist must be primarily an experimenter, familiar with the tools and procedures used in the science.

Outside of mathematics, physics and electrical engineering the training of physicists includes certain other "must" subjects. As already emphasized, the physicist in industry must be versatile and adaptable. In view of the increasing complexity of industrial materials and operations, he should also know considerably more chemistry than has been the case in the past. Indeed, I would like to suggest that the proper course of training for an industrial physicist should be one that would comprise almost as much chemistry as physics. Let me repeat once more some remarks which I made in this connection in 1937.[4]

> Chemistry is a subject which the average student of physics prefers to neglect, and he usually regards his course in this subject as an unavoidable evil. No doubt some of you have heard the witticism that a chemist is an individual who knows a little physics and wishes he knew more, while a physicist is a gentleman who knows no chemistry and thanks kind fates for this ignorance. The physicist is unconsciously snobbish with respect to chemistry. He is the inheritor of traditions which originated with the mathematical physicists of the eighteenth century. They were the supermen who intended to solve the problems of the universe by one grand differential equation. On the other hand, since the chemists are the spiritual descendants of the alchemists and similar persons of ill repute, it was but natural for the physicist to close his nostrils and incidentally, his mind, when in contact with chemists.
>
> While Ostwald first brought physics into chemistry and Willard Gibbs could visualize the essential similarity of chemical and physical reactions, the physicist has never learnt the really useful kind of thermodynamics such as is taught in any good course in physical chemistry. As a result, the physicist is singularly unaware of the differences between free and total energy or of the many useful results that have been deduced by the applications of these concepts to reactions of various kinds.
>
> Physical chemistry is a subject that ought to be *compulsory for all physicists;* the fundamental knowledge thus obtained of thermodynamics, colloid chemistry and electrochemistry will prove an invaluable asset. This ought to be accompanied by a course in inorganic chemistry and a briefer course in organic chemistry.
>
> For in industry and everywhere else outside of college, problems do not arise in general as applications of pure physics or chemistry. Physics and

[4] Reference 3, p. 65.

chemistry, as taught in college, deal with idealized observations. For pedagogic reasons we divide physics into various topics and chemistry into an equally large number of topics and segregate one group from the other, usually in different buildings. But nature is one and indivisible. In the language of physics we can describe natural phenomena in general as involving an interaction between a number of different factors, some chemical, some physical. According as one set or the other predominates we label the phenomena "physical" or "chemical." But in practice it does not pay to forget any factors that may be of importance, whatever their label.

Metallurgy is another subject about which the physicist should be better informed. In recent years considerable progress has been made in the interpretation of observations on the behavior and properties of metals in terms of electronic energy levels. To the physicist the field of metallurgy should offer attractive possibilities for the utilization of his special training. While this has received some recognition both in this country and in England, the number of physicists actually engaged in metallurgical work is still extremely small. Undoubtedly an elementary course in the physics of metals, given as part of the undergraduate training, would lead to greater interest in this field among physicists, as well as more opportunities for their employment in industry.

If the program which I have outlined in this discussion appears strenuous, I would emphasize once more the fact that, to hold his own in industry, the physicist must be familiar with a much wider range of fields which adjoin physics than heretofore and at the same time more thoroughly grounded in the basic concepts and principles which constitute classical physics.

Before concluding my remarks, I would like to stress two other matters which I believe should be part of the education of a physicist in industry.

The first matter involves a training, which need not be at all extensive, in the tools and operations used in the machine and carpenter shops, and in the preparation of intelligible drawings for use in these shops. In carrying out experiments, in the construction of devices, especially in industrial research work, a knowledge of good shop practice is of great help in achieving desired objectives; on the other hand, any attempt on the part of a Ph.D. or equally learned highbrow to dictate to shopmen anything which the latter know to be bad practice will not only result in the failure of the experiments but may often make the researcher *personâ non gratâ* in the shop. This is not to say that the shop men are always right, but that a tactful approach will usually accomplish wonders, making the impossible possible.

The final point which I should like to stress is one that is often forgotten in connection with the training not only of physicists but of scientists in general. Progress in science as well as in other branches of human endeavor depends, ultimately, upon the ability to communicate ideas to others by means of language. Whether it be the exposition of an idea by word of mouth, or the description of experimental observations in a paper for publication, clarity of expression and logical presentation of data and conclusions are prime requisites. It is not required of the scientist that he be either an orator or a clever wielder of 64-dollar words, but only that he express himself clearly in everyday language and use this language correctly.

To achieve this objective students should be asked to practice the writing of essays. The topics chosen should be such as have a direct bearing upon the fields of study. Seminars should be encouraged at which each member of a class presents a verbal report on some topic.

Whether such a course of training as that which I have outlined will require more than the regular four years is a debatable question. In view of the fact that many of those who will apply for training in the period immediately following the war are men who have already spent a year or more at the front, it will certainly be advisable to make the duration as short as possible. How to combine intensity with breadth of training will present a problem that can be solved only by actual experience.

I am more and more convinced that our happiness or unhappiness depends far more on the way we meet the events of life than on the nature of those events themselves.—KARL VON HUMBOLDT.

Physical Science Courses for Liberal Arts Students

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In general it must be agreed that science, for good or ill, plays a dominating role in our western civilization, a role which may be compared with those played by metaphysics in the classical civilization of Greece and by theology in the medieval civilization. This means not that metaphysics and theology have no part in our present civilization, but that they do not shape the majority of men's thoughts and lives to the extent which they did in the past nor to the extent which science does today. Since the time of the Renaissance the Western World has changed in many aspects and science has had much to do with these changes. If one of the functions of education is to aid in the understanding of our present civilization, science should have a place in everyone's education. This appears to be generally recognized as desirable, inasmuch as practically every college curriculum includes some science course.

Students, however, differ considerably in ability and interests. From the point of view of the science instructor students can be divided into three categories: those who intend to become professional scientists, those who intend as engineers to apply scientific principles, and those—usually a large majority—whose primary interests lie outside of the sciences. Judging from the scientific results which have been produced in this country, it would appear that the colleges and universities have been successful in training the first two groups. It is generally true that the needs of the third group are not adequately met by the first departmental course in physics or the other sciences.

It is with the third group, the nonscience major, that this discussion is concerned. The instruction of this group presents a problem, and it is necessary to make a real effort to understand the needs of these students and to formulate the objectives of a course or courses in sciences designed to satisfy these needs. These objectives should include training in scientific methods of obtaining knowledge and a widening of the intellectual horizon of the student by the presentation of a broad view of the unity and achievements of science.

The scientific method is so ingrained in every scientist that while he uses it in his scientific work he would probably have difficulty in describing it concisely. A short statement by Karl Pearson[1] on the scientific method may come as close to general acceptance as any:

> The classification of facts, the recognition of their sequence and relative significance is the function of science, and the habit of forming a judgment upon these facts unbiased by personal feeling is characteristic of the scientific frame of mind.

It is not really important that we should be able to formulate a short statement of the scientific method, but rather it is important that we should inculcate in the student an appreciation of and the habit of using such a method. Instead of presenting science in a factual manner it is better to show the method by which this knowledge was attained. The historical approach, especially through the reading of appropriate original material, not only interests the student but provides excellent illustrations of the use of scientific methods. The student sees how the facts were collected, how concepts and definitions arose, how assumptions and hypotheses were introduced and theories and physical laws were evolved. Then follows the important step of making deductions from the theory or law and determining whether these deductions can be verified by experiment. The experiments may result in modification of the theory or, in an extreme case, in the theory being discarded.

It was in making experiment the crucial test rather than in placing absolute faith in theory that Galileo clashed with his contemporaries. Galileo's contribution to science was very considerable, for he not only obtained important scientific results but left to the world a book[2] that provides a model of clear scientific thinking.

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[1] K. Pearson, *Grammar of science* (Everyman's Library), p. 11.

[2] Translated by H. Crew and A. de Salvio under the title *Two new sciences* (Macmillan, 1914).

Let us consider a brief passage from this book on 'Naturally Accelerated Motion.'[3]

> And first of all it seems desirable to find and explain a definition best fitting natural phenomena. For anyone may invent an arbitrary type of motion and discuss its properties; but we have decided to consider the phenomena of bodies falling with an acceleration such as actually occurs in nature and to make this definition of accelerated motion exhibit the essential features of observed accelerated motions.

We are all familiar with the sequel, in which Galileo rolled a ball down an inclined plane and found that the relationship between distance traveled and time taken agreed with the deductions.

A simple illustration in which the deductions did not agree with experiment is to be found in the now discarded caloric theory. An attempt to interpret these discrepancies eventually led to the theory of heat as a form of energy. Agreement with evidence forms the crucial test of any scientific theory. Such illustrations should be used to show the student that a theory is not something static or sacred, but may be modified when the evidence demands it. It is important to point out the interplay of induction and deduction in science, for this goes far in making the scientific method real and alive.

Since the scientific method can be illustrated by any one of the sciences, as is done so well for physics by Professor Taylor's book,[4] or even by taking a small section of a single science, we may ask why one should give the liberal arts student a general course in the physical sciences rather than a course in physics? The answer to this question comes from a consideration of our second objective. A general course, comprising mathematics, physics, chemistry, astronomy and geology, widens a student's intellectual horizon by giving him a background of the scientific culture of our civilization and by permitting him to grasp the interrelationships of these sciences.

Many important illustrations can be used to show the student how results in one of the sciences have had profound effects in some other of the sciences. The work of Tycho Brahe and Kepler was fundamental to that of Newton. Radioactivity has been of great value to the geologist in determining the age of rocks. Examples such as these enable the student to gain a unified picture of the physical world—an internally consistent picture in which the disciplines and facts of the various sciences mesh one into the other.

A common criticism made against these general courses is that they are apt to be superficial. Superficiality may arise from poor selection of topics, from a lack of integration or from an inadequately prepared staff. Selection of topics is difficult, and frequently far too much material is presented. With lack of integration a general course may degenerate into a series of short courses in the various sciences and so lose its opportunity to show the unity of the sciences. It is difficult to find instructors well trained in all the physical sciences, but usually an instructor well trained in one can gain the essentials of the others with interdepartmental cooperation. Without enthusiasm and cooperation these courses are almost bound to fail.

Associated with the problem of selection of topics is that of the degree of analysis to be attempted. Obviously the degree of analysis and amount of problem solving must be far less than that required in a full course in one of the sciences. It is more important for these students to know that if the speed of a car is doubled its kinetic energy and stopping distance for a constant braking force are quadrupled, than it is for them to be able to compute the number of dynes or poundals required to stop the car. The type of quantitative work in which proportional values are used frequently means much more to the student in terms of his experience than that demanding absolute values. In some cases absolute values may be required, in others relative values and in still others purely qualitative answers may be more suitable. The choice depends on the subject matter, the time available and frequently on the students' mathematical ability.

To present this material to the student we have used, at the University of Chicago, three lectures, in groups of over 200 students, and one discussion, in small groups of about 25 students, per week. As a result of this experience, we believe that more intimate contact with the

[3] Reference 2, p. 160.
[4] L. W. Taylor, *Physics, the pioneer science* (Houghton Mifflin).

student would be achieved and consequently more adequate presentation of the material might be possible if there were two lectures and two discussions per week. This we are going to put into effect next year. The lectures are illustrated with many experiments, lantern slides and motion pictures specially designed for the course.

A problem arose with regard to providing the student with laboratory experience. Instead of following the conventional method we have used a demonstration laboratory which was built up by Professor Lemon, and has been described elsewhere in some detail.[5] It is our experience that this device gives the student who takes this course first-hand experience with the phenomena more adequately than does the conventional laboratory. In order to supplement the astronomy lectures the students are taken in small groups to view some of the celestial objects in a 6-in. refracting telescope. We are also fortunate in being able to give one or two special lectures at the Adler Planetarium, where the annual motion of the sun among the stars, the motions of the planets and many other astronomical phenomena can be clearly presented. In chemistry we have a small laboratory in which are provided a few simple and fundamental chemical experiments for those students who feel the need.[6] Prior to the onset of the present transportation difficulties we had one or two expeditions to places of geologic interest near Chicago. These always interested the students very much.

Short weekly quizzes are given during the discussion sessions, and a 1-hr quarterly quiz. These are used for the student's own information to show him how well he is assimilating the material. His grade is based solely on a 6-hr comprehensive examination, which permits us to test for a knowledge of the interrelationships of the sciences as well as of the individual sciences themselves. The examination is divided into sections, testing for information, application of physical principles and understanding of physical theories.

The section on information tests the ability of the student to recall important facts and principles of the physical world, the ability to give a rough estimate of the order of magnitude of important physical quantities and the ability to recognize a precise definition of fundamental terms or concepts. The section on the application of principles is divided into the abilities to make qualitative and quantitative predictions by using the principles of science, to make predictions at the same time that reasons are given for them, to interpret a set of data by making predictions and to cite the data upon which a prediction depends. Section three, on the understanding of theories, tests the abilities to distinguish among statements of observation, theoretical statements and definitions, to cite the experimental evidence supporting a given theory and to explain phenomena in terms of a theory. The examinations are carefully analyzed and are useful in revealing to the instructors what portions of the course are in need of revision.

Some time ago we were interested in determining how much the students retained of the course over a period of time. Students who had taken the course half a year, one and a half years and two and a half years previously were asked to volunteer to take a 2-hr examination on the material covered by the course. Until they arrived for the examination they were unaware of what type of subject matter was to be covered, so that no review was possible. The grades obtained on this examination were then compared with their earlier comprehensive grades. The results showed that there was a progressive decrease in grades with the length of time elapsed since the course was taken, but it was not nearly as large as we might have expected. It was interesting and gratifying to see that there was more retention in the sections on application of principles and understanding of theories than in the section on information. Our experience leads us to the conclusion that these general science courses can present to the student the methods, unity and achievements in the physical sciences in such a manner that the important aspects become a permanent part of his cultural background.

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[5] H. B. Lemon and Fitz-Hugh Marshall, *The demonstration laboratory at the University of Chicago* (Chicago, 1939).
[6] H. I. Schlesinger, J. Chem. Ed. **12**, 524 (1935); digest, Am. J. Phys. **4**, 55 (1936).

Demonstration of the Doppler Effect

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A DISCUSSION of the Doppler effect is often accompanied by a demonstration in which a reed is maintained in vibration on the end of a rod rotating with constant angular velocity about a vertical axis perpendicular to the length of the rod. As the reed moves toward a stationary observer the observed pitch increases, while as the reed moves away this pitch is lowered. Unfortunately, experience has indicated that the demonstration is not always convincing. The pitch does not change abruptly, but varies continuously from its highest to its lowest value. If the rate of rotation is slow the change in pitch is small and often escapes an untrained observer. On the other hand, if the rotational velocity is increased a confusing phenomenon is introduced because of the rise and fall in intensity as the reed approaches and recedes from the observer.[1] This makes it difficult to concentrate on the changes in pitch which are occurring with the same rotational speed.

Most of these difficulties can be avoided by making use of beats[2] to detect a small change in the observed frequency of a moving source—changes caused by velocities of the order of 40 or 50 cm/sec when the sound-source frequency is 1000 cycle/sec. In the simplest arrangement two tuning forks having the same frequency vibrate in unison: One is stationary and the other is hung as a pendulum bob (Fig. 1) by a suitable suspension, preferably bifilar. If the suspended fork is set swinging through an appreciable arc in the plane of a stationary observer, he readily hears the beats set up between the wave trains from the moving and stationary sources. A 1000-cycle/sec fork on a 1-m suspension, swinging through an arc of total length 1 m, yields approximately three beats per single swing. If the two forks emit sounds of approximately the same intensity, the beats are well defined and easily observed.

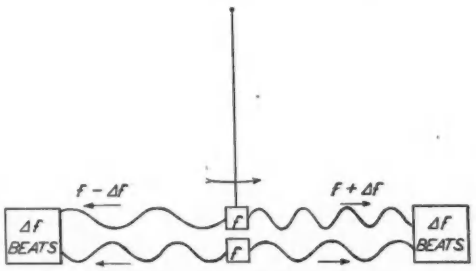

FIG. 1. The two sound sources have the same frequency f. The number of beats is approximately the same on backward and forward swings.

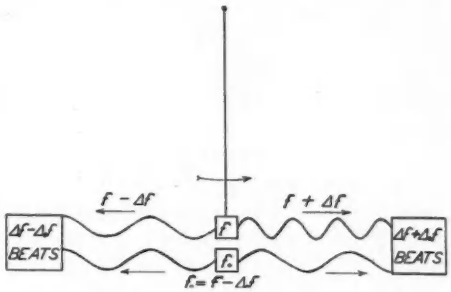

FIG. 2. Sources initially adjusted to have a frequency difference $\Delta_0 f$. The beat frequencies are the sum and difference of $\Delta_0 f$ and Δf, where Δf is the Doppler frequency shift.

Although the velocity of the moving source is continuously changing, this is not a serious objection under the conditions of the experiment. The resultant change in frequency, or number of beats per second (or swing), is determined by the average velocity during a single swing. In fact, this variable velocity can be a distinct advantage for, by using longer suspensions and correspondingly larger pendulum amplitudes, one can increase the time of a single swing and the number of beats per swing so that the change in the beat frequency is easily observed as the velocity changes during the swing. The rapid succession of beats when the pendulum is going through its equilibrium position and the slowing

[1] These bad features of the method have also been pointed out by J. Zeleny, Am. J. Phys. 9, 174 (1941).

[2] The use of beats to demonstrate the Doppler effect is not new; see Rudolph Koenig, *Quelques expériences d'acoustique* (Paris, 1882), p. 41. The present method, however, differs from either of those employed by Koenig.

down of the beat as the pendulum velocity drops near the end of the swing bring out the dependence of the Doppler frequency shift upon the velocity of the moving source.

There are, however, two factors that might cause difficulty when impact-excited tuning forks are used in the demonstration. The first is the difficulty of imparting to the forks approximately equal amplitudes; the second is the limited time available for observation after the forks are struck. With massive forks having low decrements these difficulties are not so noticeable. Actually, after the first few trials we used two small loudspeaker units instead of the tuning forks. In this case it is quite easy to adjust the sound sources to equal intensities so as to insure well-defined beats. Of course, electrically maintained tuning forks would serve as well; the wire leads to the swinging unit make convenient bifilar suspensions.

The demonstration as described so far does not differentiate between the increased frequency resulting from the motion of the source toward the observer and the decreased frequency due to motion away from the observer. This can be done in a very simple manner as indicated in Fig. 2. The prongs of the stationary fork are loaded with small pieces of wire or wax or with rubber bands until its frequency is reduced 4 or 5 cycle/sec. When speakers are used this can be done if two audiofrequency oscillators are available and one of these has a continuously variable frequency control. The effect is now a beat frequency $\Delta_0 f$ when the pendulum is stationary; when the pendulum swings toward the observer this increases to $\Delta_0 f + \Delta f$, where Δf is due to motion of the pendulum; when the source recedes it drops to $\Delta_0 f - \Delta f$. Thus, using 1000-cycle/sec sources, if the suspension and the arc are each 1 m, and the initial frequency difference was set at four beats per second, there will be one beat as the pendulum swings away from the observer and seven as it swings towards him. This provides a rather striking demonstration, and one that is even more interesting if observers are stationed in two groups on opposite sides of the moving source. When one group hears the high beat frequency the other hears the low.

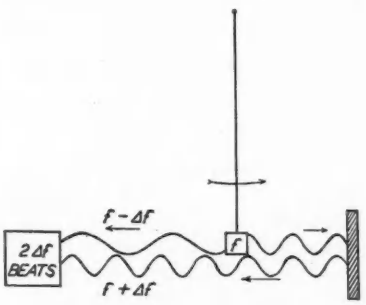

FIG. 3. Beats between direct and reflected wave trains emitted by a moving source.

Beat Phenomena with a Single Moving Source

Many modifications of the foregoing method can be used. One that works well is to utilize a reflector placed perpendicular to the plane of oscillation of the pendulum. By reflection one has a virtual source moving in the opposite direction to the actual moving source, as shown in Fig. 3. The result is a beat between the direct and reflected sound waves.[3] This beat frequency is twice that resulting from the superposition of waves from a stationary and a moving source for equal velocities. In all of the discussions it has been assumed that the velocity of the source is small compared with the velocity of sound and that $f_0/(1 \pm v/c)$ is equal to $f_0(1 \mp v/c)$ to sufficient accuracy. Here f_0 is the actual frequency of the moving source, v is the velocity of the source relative to the stationary observer and c is the velocity of sound.

[3] However, as has been pointed out by Heaps [Am. J. Phys. 9, 313 (1941)], when the fork is moving toward the wall or away from it it is true that the beats are present and readily observed. But even with the fork at rest the direct sound and the reflected sound give rise to maximums and minimums in positions which depend on the distance of the fork from the reflector, and when the fork is moved the frequency at which these maximums and minimums pass the observer is the same as the frequency of the Doppler beats. See also J. O. Perrine, Am. J. Phys. 12, 23 (1944); and G. F. Herrenden Harker, Am. J. Phys. 12, 175 (1944).

NOTES AND DISCUSSION

A Laboratory Method for Exploring the Field Intensity about a Magnetic Pole

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EXPERIMENTS for studying the field of a magnetic pole generally involve, of necessity, the action of both poles of the magnet on the apparatus used to measure the magnetic field. In the arrangement described herein, the influence of one of the poles of the magnet is eliminated, giving in effect a single-pole magnet.

The apparatus consists of a short magnetic needle m, Fig. 1, suspended by a fiber with freedom to vibrate in a

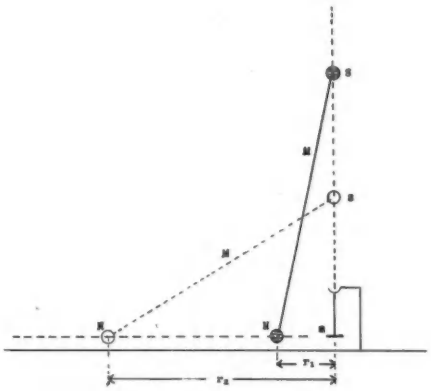

FIG. 1. Method of exploring the field intensity about a magnetic pole.

horizontal plane; a magnet M at least 50 cm long with a ¾-in. iron ball on each end; and an L-shaped wooden support and track for the magnet M.

When a magnet M is placed, as shown in Fig. 1, with one pole, say S, on the extension of the axis of vibration of the pendulum m, the influence of that pole on the period of m is eliminated, since the horizontal component of its magnetic field intensity at m is zero. The action of the other pole N on m can be measured at various distances from m by timing the oscillations in the usual way. As the pole N is moved along the horizontal track the pole S is kept on the axis of the magnetic pendulum by the wooden tracks for N and S; the tracks may be equipped with a linear scale to permit direct readings of r.

This apparatus has been used successfully in first-year college physics laboratory work. Within the limitations of the experiment Coulomb's law is confirmed, if it is assumed that the pole N is located at the center of the iron sphere at the end of the magnet. Without the spheres the pole effect is distributed and no simple inverse square rule suffices.

A Demonstration Water Hammer Made of Metal

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AN effective demonstration water hammer can be made of a cylindrical brass pipe 2 ft long and 1¼ in. in diameter. This pipe is closed by welding a brass plate to each end. A hole is drilled and threaded in one plate, and fitted with an ordinary brass stopcock.

The following procedure is probably the most convenient for introducing the water. An amount of water having approximately two-thirds the volume of the tube is put into a large beaker. The pipe is then evacuated and the stopcock closed. When the stopcock is opened *under* water, with the nozzle touching the bottom of the beaker, the proper charge of water is introduced automatically. Since it is almost impossible to close the stopcock at just the right instant, some air will re-enter the system. This must then be pumped out—with the tube held vertically, with the water at the bottom and the stopcock at the top, lest water be drawn into the pump.

This water hammer made of metal has the following advantages over the conventional models made of glass.

(1) It is virtually unbreakable.

(2) It seems much more spectacular because it produces a much louder hammer clang.

(3) It makes possible a double hammer action. When the tube, in the vertical orientation, is suddenly translated downward the water column remains behind and strikes the top with a bang. After the tube is stopped the water hits the bottom.

(4) When the vacuum is especially good the water bounces upward from the bottom and then falls to the bottom again. Thus one can get a triple hammer action.

(5) One can demonstrate directly the effect upon the hammer of different degrees of evacuation. Thus one can readmit the air to show that the hammer action disappears and then again extract any amount of it at will. For this only a few strokes of a bicycle pump are necessary.

A disadvantage is that the contents of the tube cannot be seen. However, this adds to the mystery of the situation when the apparatus is first shown.

We have a companion tube which has a slug of metal in it instead of the water. We demonstrate this first. It takes very little persuasion to convince a class of the presence of the metal slug. Then we bring out the water hammer—without mentioning water. When the class then insists that there must be a chunk of metal in that also, we open the stopcocks of both. When the one thereupon continues to hammer, while the other does not, the class really becomes interested. Finally, one can demonstrate the presence of the water by "slinging" some of it out of the tube when the stopcock is open.

Resistances in Parallel

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IN teaching a first course in physics, it is advantageous to use problems that are not complicated by long and arduous computations. This is especially true of problems involving the sum of reciprocals, such as resistors in parallel in a d.c. circuit, condensers in series in an a.c. circuit or certain applications of the lens formula. A simple method for obtaining resistances that lead to simple solutions for two resistors in parallel can be worked out as follows.

Given

$$\frac{1}{R_P}=\frac{1}{R_1}+\frac{1}{R_2}, \tag{1}$$

or

$$R_1=R_P\left(1+\frac{R_1}{R_2}\right), \tag{2}$$

let

$$R_1/R_2=R_P. \tag{3}$$

It then follows that

$$R_1=R_P(1+R_P) \tag{4}$$

and

$$R_2=1+R_P, \tag{5}$$

or

$$R_2-1=R_P. \tag{6}$$

If R_1 and R_2 as expressed in Eqs. (4) and (5) are substituted in Eq. (1), it will be seen that the sum of their reciprocals is the reciprocal of R_P.

Thus, if the values of two resistances R_1 and R_2 are such that Eqs. (3) and (6) are satisfied, the resistances, when joined in parallel, are equivalent to a single resistance R_P. Some examples are given in Table I. It may be added that the values of the resistances need not be integers. For example, if R_1 is 3.75 ohms and R_2 is 2.50 ohms, then R_P is 1.50 ohms (that is, 3.75/2.50 or 2.50−1).

This same method may be extended to include any number of resistances by using integers such that pairs of resistances are obtained which may be solved by the method just illustrated. For example, three resistances of 4, 5 and 20 ohms, respectively, may be used in parallel since they are equivalent to two 4-ohm resistances in parallel or to a 2-ohm resistance. Also, three resistances of 5, 20 and 12 ohms, respectively, may be used since the 5-ohm and the 20-ohm resistance in parallel, by the first method described, are equivalent to a 4-ohm resistance. Thus the circuit has been reduced to resistances of 4 and 12 ohms in parallel. These two resistances are then seen to be equivalent to a 3-ohm resistance. The extension of this method to include any number of resistances is thus obvious.

TABLE I. Two resistances in parallel.

R_1	R_2	R_P
6	3	2
12	4	3
20	5	4
30	6	5
.	.	.

TABLE II. Three resistances in parallel.

R_1	R_2	R_3	R_P
6	3	2	1
12	4	6	2
20	5	12	3
30	6	20	4
.	.	.	.
6	3	2	1
20	5	4	2
42	7	6	3
72	9	8	4
.	.	.	.
6	3	2	1
42	7	3	2
156	13	4	3
420	21	5	4
.	.	.	.

Additional examples of three resistances in parallel are given in Table II.

What Are Paraxial Rays?*

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IN many experimental cases in physics and in classroom derivations, the sine or tangent of an angle θ is replaced by the angle itself expressed in radians. In optics this procedure is characteristic of paraxial-ray calculations. The error involved in such an approximation is demonstrated as follows:

The well-known trigonometric series expansion of sin θ is

$$\sin\theta=\theta-\frac{\theta^3}{3!}+\frac{\theta^5}{5!}\cdots. \tag{1}$$

Generally the error may be indicated as

$$\text{Percentage error}=-100(\sin\theta-\theta)/\theta, \tag{2}$$

the negative sign being used here to put the tabulated errors in positive form. Of course, sin θ is less than θ.

Combination of Eqs. (1) and (2) gives

$$\text{Percentage error}=100\left(\frac{\theta^2}{3!}-\frac{\theta^4}{5!}\cdots\right). \tag{3}$$

But the expression in parentheses is an alternating converging series for which the remainder is less than the first term not included in summing any number of terms.

Usually percentage errors are not expressed to more than two significant figures. Therefore, when the second term is less than 1 percent of the first, only the first need be included. Now $\theta^4/5!<0.01\theta^2/3!$ when $\theta<24°$, or 0.43 radian. Thus for angles less than 24° we may write

$$\text{Percentage error}=100\theta^2/3! \tag{4}$$

From this expression it is evident that the error varies as the square of the angle. Table I, which has been calculated with the aid of an eight-place table of functions of angles, gives the percentage errors for a few values of θ.

A similar discussion is applicable to any case of the tangent for which $\theta<9°$. The upper value is necessarily less

TABLE I. Comparison of exact and approximate values.

Angle (deg.)	Percentage error $-100(\sin\theta-\theta)/\theta$ Exact [Eq. (3)]	$-100(\sin\theta-\theta)/\theta$ Approx. [Eq. (4)]	Percentage error $100(\tan\theta-\theta)/\theta$ Exact [Eq. (5)]	$100(\tan\theta-\theta)/\theta$ Approx. [Eq. (6)]
0	0.0	0.0	0.0	0.0
1	.00504	.00507	.010	.010
2	.020	.020	.041	.041
3	.046	.046	.091	.091
5	.13	.13	.25	.25
10	0.51	0.51	1.0	1.0
20	.20	.20	4.3	4.1

than in the case of the sine because of the nature of the series,

$$\tan\theta=\theta+\frac{\theta^3}{3}+\frac{2\theta^5}{15}+\frac{17\theta^7}{315}+\cdots.$$

As before, by definition,

$$\text{Percentage error}=100(\tan\theta-\theta)/\theta,$$

$\tan\theta$ being greater than θ; and

$$\text{Percentage error}=100\left(\frac{\theta^2}{3}+\frac{2\theta^4}{15}+\frac{17\theta^6}{315}+\cdots\right). \quad (5)$$

For values of $\theta<9°$ all terms but the first may be neglected as before, so that

$$\text{Percentage error}=100\theta^2/3. \quad (6)$$

For both the sine and the tangent the approximate percentage error is proportional to the square of the angle, but it is twice as great for the tangent as for the sine.

The term "paraxial rays" is a relative one and to some extent a matter of arbitrary choice. But we can arbitrarily define such rays as those for which the angle is substituted for the sine and the tangent wherever the error in doing so is not greater than 0.10 percent, whatever error may ensue in calculated results. Then we can include within the meaning of the term *paraxial rays* all meridian rays that are not inclined at an angle greater than 3° with the axis.

* A contribution of a senior student, submitted by Professor L. E. Dodd.—EDITOR.

The Synthesis of Light

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IT has been emphasized recently[1] that a narrow beam of white light which has been spread out into a spectrum by a prism cannot be re-established as a beam of white light by a second prism of the same angle and material placed in the reversed position.

In order to obtain a beam of synthesized white light, *two* additional similar prisms are required. Simply employing a third prism, as suggested in reference 1, does not accomplish this. The narrowed spectrum appearing upon emergence from the third prism becomes narrower and *apparently* converges to a point. Actually, however, there is no single point at which all the spectral colors are brought together, although within a comparatively small region the originally converging rays become diverging and the spectrum is reversed. Thus resolution and *complete* synthesis of light cannot be realized with any arrangement of only three similar prisms.

The arrangement of a lens and a slit in connection with the parallel, spectrally distributed rays emerging from the second prism, as indicated in Fig. 4 of reference 1, serves to demonstrate conclusively that the second prism does not synthesize the light which was resolved by the first one. The "image" formed by the lens, however, is not strictly a white image such as is formed when white light enters the lens. It may appear to be white because even with a very narrow initial beam of white light there is sufficient overlapping of the spectral colors to obscure the distribution that must exist. Of course, the real situation is not quite as simple as this because the focal length of the lens is not the same for all the spectrally distributed rays which enter the lens. Since it is evident, therefore, that at no point may a strictly white image be formed, complete synthesis is not achieved by the procedure indicated in Fig. 4 of reference 1.

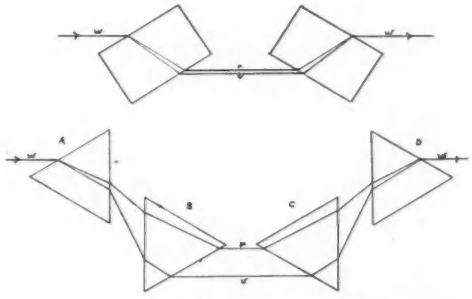

FIG. 1 (above). Resolution and synthesis by means of two similar rectangular plates.

FIG. 2 (below). Resolution and synthesis by means of four similar prisms.

As already stated, a beam of white light may be resolved and completely synthesized by means of four similar prisms. Resolution and synthesis may be brought about also by means of two similar rectangular blocks. Figure 1 of the present note represents the disposition of two similar plates. The symmetrical arrangement of the four prisms in Fig. 2 shows it to be equivalent to that of the two plates except for the greater separation of the resolved rays in the case of the prisms. In Figs. 1 and 2 the indices of refraction were taken to be somewhat larger than is usually encountered with glass. Also, the diverging angles are larger than would be observed experimentally. However, the diagrams do bring out clearly how both resolution and synthesis are accomplished.

[1] Perkins, Am. J. Phys. 9, 188 (1941).

Uniform Circular Motion. How Should It Be Taught?

VOLA P. BARTON
Goucher College, Baltimore, Maryland

PROFESSOR JONES[1] has given a clear discussion of two very common methods of teaching the subject of motion with constant speed in a circle; but the implied conclusion as to the proper method of presenting the subject in my judgment should be questioned.

The note referred to comments on teaching uniform circular motion as a case of statics, using d'Alembert's principle, and also on teaching it as a case of accelerated motion caused by a centripetal force. It favors the use of the laws of statics because to a student not knowing much about physics the solution by this method seems simpler and more straightforward.

This may be a correct reason, but in my judgment it is not a valid reason for treating uniform circular motion as a case of statics to a beginner. If the purpose in teaching physics is to get the correct answer to a given type of problem in the simplest manner, the foregoing approach may be good. But if the purpose in teaching physics is to help the student to understand the world in which he lives, it should be pointed out that when a body moves in a circle with constant speed, there must be acting on that body a resultant force of constant magnitude towards the center of that circle, the centripetal force. By treating this motion as a case of accelerated motion, it is emphasized that the body is not in equilibrium. According to my experience the ordinary student needs to have this emphasis made, if he is to understand the problem.

It is true that either method yields the correct answer to a problem on this subject. It is also true that if one fully understands either method, one's physical concepts are correct. But in my judgment, the beginner gains a much clearer idea of the forces involved and of the kind of motion that is actually taking place, if this motion is treated not as a case of statics, but as one of accelerated motion.

[1] A. T. Jones, Am. J. Phys. 11, 299 (1943).

What Is Centrifugal Force?

OSWALD BLACKWOOD
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PROFESSOR JONES has suggested[1] that "we cease using the term centrifugal force to mean the reaction to a centripetal force and that when we employ the term at all we use it consistently to mean a force which we imagine applied to the body itself in order to treat the problem by the law of statics" (d'Alembert's principle).

Most of us will agree that the general college student is more familiar with the term centrifugal force than with its sister term centripetal force. We cannot avoid the issue when he asks the meaning of the former term. If we tell him that a centrifugal force is an imaginary, virtual force introduced in order to treat a problem by the law of statics, we ask him to imagine that when he is riding in an automobile weaving through traffic, the car is in equilibrium but the street and the earth itself are twisting and squirming in a most astonishing manner. The common sense of the thoughtful student will be outraged by this suggestion, and he may be encouraged to believe that physics, though a marvelous subject, is too abstruse for his comprehension. On the other hand, the statement that a centrifugal force is merely a reaction force, such as he has studied in linear mechanics, will fit new facts into familiar categories. Moreover, by thus dealing with centrifugal force, we have a welcome opportunity to teach again the important fact that, in accord with Newton's third law of motion, a reaction force does not act on the body that is accelerated but on some other body. Thus when an automobile goes around a curve, the side of the car exerts a *centripetal* force on the passenger, while he exerts an equal but opposite *centrifugal* force on the side of the car.

It is difficult enough for a beginner to understand large numbers of necessary concepts without being confused by the use of unreal, virtual forces.

[1] A. T. Jones, Am. J. Phys. 11, 299 (1943).

Centrifugal Force Again

ARTHUR TABER JONES
Smith College, Northampton, Massachusetts

I AM heartily in sympathy with Professor Blackwood's desire to make as little confusion as possible in the minds of our students. It is precisely for this reason that I favor giving up the use of the term "centrifugal force" with the meaning of a reaction to a centripetal force. If a student goes on into more advanced work in physics or into engineering he is likely to meet a centrifugal force when he deals with rotating frames of reference, and in engineering work he will find centrifugal forces applied, without apology, to the body that describes the curved path instead of to the body that produces the constraint. If he has been taught that a centrifugal force is the reaction to a centripetal force, and that a force and its reaction act always on different bodies, is not confusion likely to arise at this point? Is he not likely to think that one of the books he studies is wrong?

I also agree fully with Professor Barton that in the introductory course in physics we should treat circular motion as a case of accelerated motion, and that we should employ the idea of a centripetal force. In the introductory course I propose that we (1) make use of centripetal forces, (2) point out that these forces, like all forces, have reactions, (3) state that the term "centrifugal force" is sometimes applied to the reaction to a centripetal force, but (4) add that it seems better to reserve this term for a different meaning which the student will probably meet later if he goes farther into physics or engineering.

Book-Length Biographies of Physicists and Astronomers

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Illinois Institute of Technology, Chicago, Illinois

THE individual biographies and collections listed below comprise an addendum to the writer's earlier paper of the same title.[1] The author is indebted to interested readers[2] of this paper for furnishing him with a number of the items listed. The remaining titles, roughly two-thirds of the total, derive from a packet of slips, misfiled under "engineers" and turned up in the course of preparing the manuscript of the fourth and last of the writer's series of lists of book-length biographies: of physicists and astronomers,[1] of chemists,[3] of mathematicians,[4] and of engineers, metallurgists and industrialists.[5]

PHYSICISTS—INDIVIDUAL

C. Babbage, *Passages from the life of a philosopher* (Longmans, Green, London, 1864).

I. Levine, *Francis Bacon (1561–1626)* (L. Parsons, London; Small, Maynard, 1925).

F. Steegmüller, *Sir Francis Bacon, the first modern mind*, by Bryon Steel (pseud.) (Doubleday, Doran, 1930).

H. S. Redgrove, *Roger Bacon, the father of experimental science and mediaeval occultism* (W. Rider, London, 1920).

Sir J. C. Bose, a sketch of his life and career; with an appendix by P. Geddes entitled, "The Indian Temple of Science" (G. A. Natesan, Madras, 1918).

T. Farrington, *A life of the Honourable Robert Boyle, F.R.S., scientist and philanthropist* (Guy and Co., Cork, 1911).

W. R. Burgess, *Michael Faraday* (London, 1877).

S. Martin, *Michael Faraday: philosopher and Christian. A lecture* (Hotten, London, 1867).

T. Martin, *Faraday* (Duckworth, London, 1934).

W. C. Bruce, *Benjamin Franklin, self-revealed; a biographical and critical study based on his own writings* (Putnam, 1917; ed. 2, 1923; ed. 3, 1942), 2 vol.

S. G. Fisher, *The true Benjamin Franklin* (Lippincott, 1899). Many later editions; reissued as *Benjamin Franklin* (1927).

J. B. McMaster, *Benjamin Franklin as a man of letters* (Houghton-Mifflin, 1887).

J. E. Drinkwater Bethune, *Life of Galileo Galilei; with illustrations of the advancement of experimental philosophy* (W. Hyde, Boston, 1832; G. Dearborn, New York, 1835).

G. McColley, *The defense of Galileo of Thomas Campanella*, tr. and ed. by McColley (Smith College, 1937).

F. S. Taylor, *Galileo and the freedom of thought* (Watts, London, 1938).

O. Lodge, *Letters from Sir Oliver Lodge, psychical, religious, scientific, and personal*, comp. and annotated by J. A. Hill (Cassell, London, 1932).

G. Boole, *An address on the genius and discoveries of Sir Isaac Newton delivered . . . at the Lincoln and Lincolnshire Mechanics Institution* (Gazette Office, Lincoln, 1835).

D. Brewster, *The life of Sir Isaac Newton* (J. Murray, London; J. Harper, New York, 1831, 1835; Harper Bros., 1842; rev. and ed. by W. T. Tegg, W. Tegg and Co., London, 1875). The two-volume *Memoirs* (1855) is an expansion of this life.

H. (Lord) Brougham, *Life of Sir Isaac Newton* (Library of Useful Knowledge, London, 1829).

B. le Bovier de Fontenelle, *The Eulogium of Sir Isaac Newton* (J. Tonson, London, 1728).

B. le Bovier de Fontenelle, *The life of Sir Isaac Newton; with an account of his writings* (J. Woodman and D. Lyon, London, 1728).

G. Grant, *The life of Sir Isaac Newton, containing an account of his numerous inventions and discoveries; and a brief sketch of astronomy previous to his time, compiled from authentic documents* (J. M. Glashan, Dublin, 1849).

E. F. King, *A biographical sketch of Sir Isaac Newton . . . to which are added authorized reports of the oration of Lord Brougham (with his Lordship's notes) at the inauguration of the statue at Grantham; and of several of the speeches delivered on that occasion [by W. Whewell, Sir B. C. Brodie, Rev. J. W. Turner, and T. Winter]* (Simpkin, Marshall, Hamilton, Kent, London, 1858).

V. E. Pullin, *Sir Isaac Newton, a biographical sketch* (Benn, London, 1927).

S. P. Rigaud, *Historical essay on the first publication of Sir Isaac Newton's Principia* (Oxford Univ. Press, 1838). Somewhat mistitled; contains much interesting material on Newton's character and disposition.

Correspondence of Sir Isaac Newton and Professor Cotes, including letters of other eminent men, now first published from the originals in the library of Trinity College, Cambridge; together with an appendix containing other unpublished letters and papers by Newton; with notes, synoptical view of the philosopher's life, and a variety of details illustrative of his history by J. T. Edleston (J. W. Parker, London, 1850). For references to other correspondence of Newton see G. J. Gray, *A bibliography of the works of Isaac Newton; together with a list of books illustrating his works, with notes* (Bowes and Bowes, Cambridge, ed. 2, 1907).

J. Chevalier, *Blaise Pascal*, tr. by L. A. Clare (Sheed and Ward, London; Longmans, Green, New York, 1930).

Speeches delivered at a dinner held in University College, London, in honour of Professor Karl Pearson, 23 April, 1934 (Privately printed, Cambridge Univ. Press, 1934).

H. C. Bolton, *Scientific correspondence of Joseph Priestley . . . together with copious biographical, bibliographical and explanatory notes* (Collins Printing House, Philadelphia, 1892).

J. Corry, *The life of Joseph Priestley . . . with critical observations on his works* (Wilks, Grafton, Birmingham, 1804).

J. T. Rutt, *Life and correspondence of Joseph Priestley* (R. Hunter, London, 1831–32), 2 vol. Chiefly on Priestley's theological work; contains hundreds of his letters.

H. Ware, *A memoir of the life of Joseph Priestley. Introductory to views of Christian truth, piety and morality selected from the writings of Joseph Priestley* (Cambridge, Mass., 1834).

The Priestley memorial at Birmingham, August, 1874 (British and Foreign Unitarian Association, London, 1875).

J. Renwick, *Life of Benjamin Thompson, Count of Rumford* (J. Kennett, London; Hilliard, Gray, Boston, 1848; Little, Brown, 1851; Harper, various later editions). Comprises vol. 5, series 2 of *The Library of American Biography*.

ASTRONOMERS—INDIVIDUAL

D. F. J. Arago, *History of my youth*, tr. by Rev. Baden Powell (Longmans, Green, London, 1862).

F. Baily, *Journal of a tour in the unsettled parts of North America in 1796 and 1797, with memoir* [of Baily] *by John Herschel*, ed. by A. DeMorgan (Baily Bros., London, 1856). An 1845 edition, not edited (?) by DeMorgan, is also to be found.

E. S. Holden, *Memorials of William Cranch Bond, Director of the Harvard Observatory 1840–1859 and of his son George Phillips Bond, Director of the Harvard Observatory 1859–1865* (C. A. Murdock, San Francisco; Lemcke and Buechner, New York, 1897).

M. Brod, *The redemption of Tycho Brahe*, tr. by F. W. Crosse (Knopf, 1928). A biographical novel.

Nicholas Copernicus (*Nikolaj Kopernik*) *1473–1543*. Ed. by Dr. J. Rudnicki, tr. by B. W. A. Massey (Copernicus Quatercentenary Celebration Committee, London, 1943).

G. Hort, *Dr. John Dee: Elizabethan mystic and astrologer* (W. Rider, London, 1922).

H. Macpherson, *Herschel* (Soc. for Promoting Christian Knowledge, London, 1919).

P. C. Headley, *The astronomer and soldier, the illustrated life of General Mitchel* (G. A. Leavitt, New York, 1870).

J. Renwick, *David Rittenhouse* (J. Kennett, London; Hilliard, Gray, Boston, 1848; Little, Brown, 1851; Harper, various later editions). In vol. 7, series 1, of *The Library of American Biography*.

B. Rush, *An Eulogium, intended to perpetuate the memory of David Rittenhouse* . . . (J. Ormrod, Philadelphia, ed. 3, 1795).

W. L. Webb, *Brief biography and popular account of the unparalleled discoveries of T. J. J. See* (W. Wesley and Son, London; T. P. Nichols and Son, Lynn, Mass., 1913).

PHYSICISTS AND ASTRONOMERS—COLLECTED

C. L. Brightwell, *Annals of industry and genius* (Nelson, London, 1864). Brahe and Franklin among others.

J. Cottler and H. Jaffe, *Heroes of civilization* (Little, Brown, 1931). Copernicus, Galileo, Huygens, Newton, Watt, Davy, the Curies, Einstein among others. Parts 2 and 4 of this text were reissued in 1932 as *Heroes of science*.

A. Defries, *Pioneers of science: seven pictures of struggle and victory* (Routledge, London, 1928). Bose, F. Bacon, Davy, Faraday.

E. Dunkin, *Obituary notices of astronomers, fellows and associates of the Royal Astronomical Society written chiefly for the annual reports of the council* (Williams and Norgate, London and Edinburgh, 1879). Becher, D'Arrest, Delaunay, Denison, Donati, Heis, W. Herschel, J. F. Herschel, Laugier, LaVerrier, von Littrow, Main, Mathieu, Maury, Middlemist, Murchison, Robertson, Rutherford, Ryan, Santini, Sedgwick, Sheepshanks, Strange, Waugh.

H. C. Ewart, *Heroes and martyrs of science* (W. Isbister, London, 1886). R. Bacon, Galileo, Kepler, Newton and Papin.

M. Gumpert, *Trail-blazers of science; life stories of half-forgotten pioneers of modern research*, tr. by E. L. Shumann (Funk and Wagnalls, 1936). Copernicus, Kepler, Mayer among others.

D. B. Hammond, *Stories of scientific discovery* (Cambridge Univ. Press, 1923, '24, '28, '33). Priestley, Rumford, W. Herschel, Faraday, the Curies among others.

E. Hubbard, *Little journeys to homes of great scientists* (Roycrofters, East Aurora, N. Y., 1905), 2 vol.; reissued as *Great Scientists* (vol. 12 of *Little journeys to the homes of the great*, W. Wise, New York, 1916). Copernicus, Galileo, Newton, W. Herschel, Tyndall among others.

J. N. Leonard, *Crusaders of chemistry; six makers of the modern world* (Doubleday, Doran, 1930). R. Bacon, Priestley, Boyle, Cavendish among others.

B. Martin, *Biographia Philosophica. Being an account of the lives, writings, and inventions of the most eminent philosophers and mathematicians who have flourished from the earliest ages of the world to the present time* (W. Owen and B. Martin, London, 1764). Sketches of the lives of some 150 men, including Greek and early English scientists not mentioned in more modern books.

S. P. Rigaud, *Correspondence of scientific men of the seventeenth century, including letters of Barrow, Flamsteed, Wallis, and Newton, printed from the originals in the collection of the Right Honourable the Earl of Macclesfield* (Oxford Univ. Press, 1841), 2 vols. See also, *Contents of the correspondence of scientific men of the seventeenth century, printed . . . under the superintendence of the late Professor Rigaud, comp. by Augustus DeMorgan* (Oxford Univ. Press, 1862).

A. Williams-Ellis, *Men who found out, stories of great scientific discoverers* (G. Howe, London, 1929; Coward-McCann, 1930). Some Greeks and Romans, Galileo, Faraday, the Curies among others. A child's book.

J. J. Walsh, *Catholic churchmen in science: Sketches of the lives of Catholic ecclesiastics who were among the great founders in science* (American Ecclesiastical Review, The Dolphin Press, Philadelphia, vol. 1, 1906; vol. 2, 1909; vol. 3, 1917). Vol. 1: Copernicus, B. Valentine, Abbe Häuy. Vol. 2: Regiomontanus and interesting chapters on "Clerical Pioneers in Electricity" and "Jesuit Astronomers." Vol. 3: R. Bacon.

H. C. Wright, *Children's stories of the great scientists* (Scribner, 1888, ed. 2, 1902). Galileo, Kepler, Newton, Franklin, Herschel, Rumford, Davy, Faraday, Tyndall, Kirchhoff.

T. Young, *Biographies of men of science*, ed. by G. Peacock (J. Murray, London, 1855). Cavendish, Rumford, Ingenhousz, Robison, Coulomb, Borda, Lagrange, Dollond, Maskelyne, Lelande, and Atwood among others. Vol. 2 of Young's *Miscellaneous Works*.

Home Study Circle Library: Vol. 1, *The world's great scientists* (Doubleday and McClure, New York, 1900). Galileo, Newton, Franklin, Davy, Faraday, Tyndall among others.

Lives of eminent persons: consisting of Galileo, Kepler [*by J. E. Drinkwater, afterwards Bethune*], *Newton* [*from the French of J. B. Biot by Sir. H. Elphinstone*], . . . *C. Wren* [*by C. H. Bellenden Ker*] . . . (Baldwin and Cradock, London, 1833).

SELECTED ENGINEERS AND CHEMISTS

J. Davy, *Fragmentary remains, literary and scientific, of Sir Humphrey Davy, with a sketch of his life and selections from his correspondence* (J. Churchill, London, 1858).

J. Davy, *Memoirs of the life of Sir Humphrey Davy, Bart* (Longman, Rees, Orme, Brown, Green and Longman, London, 1836), 2 vols. The biography comprising Vol. 1 of the *Collected works of Sir Humphrey Davy* is a preliminary draft for this two-volume work.

H. Mayhew, *The wonders of science; or, young Humphrey Davy (the Cornish apothecary's boy, who taught himself natural philosophy, and eventually became a president of the Royal Society)* (Harper, 1856).

J. H. Greusel, *Thomas A. Edison: the man, his work and his mind* (Arts and Crafts, Los Angeles, 1913).

F. A. Jones, *Thomas Alva Edison; sixty years of an inventor's life* (T. Y. Crowell, 1908).

F. Rolt-Wheeler, *Thomas A. Edison* (Macmillan, 1915).

W. A. Simonds, *A boy* [Francis Jehl] *with Edison; with an introduction by F. Jehl* (Doubleday Doran, 1931; Sun Dial Press, 1940).

Edison and his inventions, including the many incidents, anecdotes, and interesting particulars connected with the life of the great inventor, ed. by J. B. McClure (Rhodes and McClure, Chicago, 1879).

J. A. Ewing, *The man of room 40; the life of Sir [James] Alfred Ewing* (Hutchinson, London, 1939).

J. Trowbridge, *Samuel Finley Breese Morse* (Small, Maynard, 1901).

D. O. Woodbury, *Beloved scientist: Elihu Thomson, a guiding spirit of the electrical age* (McGraw-Hill, 1944).

Elihu Thomson, *Eightieth birthday celebration at the Massachusetts Institute of Technology* (Technology Press, Cambridge, 1933).

R. A. Taylor, *Leonardo the Florentine; a study in personality* (Richards Press, London, 1927).

T. H. Marshall, *James Watt* (L. Parsons, London; Small, Maynard, 1925).

J. P. Muirhead, *Correspondence of the late James Watt on his discovery of the theory of the composition of water, with a letter to his son* (J. Murray, London, 1846).

J. P. Muirhead, *The life of James Watt, with selections from his correspondence* (J. Murray, London, 1858; Appleton, 1859). Abridged from the three-volume *Life*.

[1] Am. J. Phys. **12**, 31 (1944). Errata: page 34, A. Gray, *Lord Kelvin . . .* ; page 36, C. R. Gibson, *Heroes of Science:* add R. Bacon, Brahe, Kepler, Priestley, Cavendish, W. and C. Herschel, Dalton, Davy, Kelvin, Maxwell, J. J. Thomson.

[2] I am particularly indebted to Mr. Sheldon Brown, of the Department of Physics, University of California at Los Angeles, for several lengthy letters.

[3] Sch. Sci. and Math., awaiting publication.

[4] Am. Math. Mo. awaiting publication.

[5] Unpublished.

Electric Current *versus* Electron Drift

ARTHUR TABER JONES
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MANY teachers have felt it unfortunate that the sense chosen for an electric current is such as to suggest that the positive charge does most of the flowing. The choice seems, however, to be so firmly established that a great deal of confusion would result if the convention were to be changed. The present note is written in the hope that the suggestion which it contains may prove to be a happy solution of the difficulty.

It has recently been pointed out[1] that in a widely known book by Nilson and Hornung[2] the sense of the electron drift is taken as the sense of the electric current. In this book the arrows which represent electric currents point in the direction of motion of the electrons, and the rules for the sense of the magnetic field around an electric current, the sense of an induced electromotive force, and so forth, are modified to fit this convention. The authors are careful to point out clearly that their convention is not the customary one.

In several places Nilson and Hornung make use of the expression "electron drift." This term seems to me an admirable one, and one that might go far toward solving our present difficulty if it were generally adopted and consistently employed. The word "drift" suggests a slow advance like that of electrons in a current-carrying conductor.

I suggest that instead of an "electron *current*" we speak always of an "electron *drift*." We can then reserve the term "electric current" for the conventional current, which has a sense opposite to that of the electron drift. Thus in a triode we should say that the current is directed from the plate toward the filament, whereas the drift is directed from the filament toward the plate.

If this terminology were adopted we should no longer have the confusion of talking about a conventional current and an electron current, with the necessity of distinguishing between the two "currents." We could build up various rules regarding drift, the terms "current" and "drift" could be used simultaneously as long as we wished to do so—and without undue confusion—and it is entirely possible that in time the term "current" might die a natural death, and leave us with a "drift" that really expresses the sense of the electron flow.

As an appendix to this note it may not be out of place to quote from S. Hunter Christie the following statement on the significance he attributed to the term "electric current." In the paper in which Mr. Christie described the network that came later to be called a Wheatstone bridge circuit he says,[3] "I adopt the term electric current, or current of electricity, as a convenient mode of expression; and by it, I would be understood merely to imply that a conductor is in a peculiar state, manifested by certain effects produced on a magnetized needle; but I would not be considered as adopting any theoretical views on the subject:—whether this state consists in the *motion* of a peculiar fluid in the conductor, in the arrangement of one previously existing, or in the excitement of a power previously latent, I am not at present inquiring."

[1] S. W. Cram, Am. J. Phys. **12**, 111 (1944).

[2] A. R. Nilson and J. L. Hornung, *Practical radio communication* (McGraw-Hill, ed. 2, 1943).

[3] S. H. Christie, Phil. Trans. **123**, 96 (1833).

REGIONAL MEETINGS

Cincinnati Meeting, June 22 to 24, 1944

The American Association of Physics Teachers met jointly with the Society for the Promotion of Engineering Education at the Netherland Plaza Hotel, Cincinnati, Ohio, June 22, 23 and 24, 1944. PROFESSOR J. G. POTTER was chairman of the program committee.

Two sessions, chiefly of concern to physicists, were devoted to discussions of experiences and findings in the teaching of college physics in the Army and Navy college training programs and to discussions of problems and plans for the teaching of physics in the immediate and postwar period. In two other sessions the physicists enjoyed the participation of the Electrical Engineering Division of SPEE. One of these sessions, arranged under the auspices of the Electrical Engineering Division, included a dinner followed by papers provoking lively discussions on questions of physics teaching. The other, under the auspices of the physics group, was devoted to the problem of providing students with a background for the pursuit of the advancing field of ultra-high frequencies. Additional sessions devoted to subjects of concern to all divisions of SPEE and to members of the Association included papers on professional consciousness, student guidance and engineering education after the war. The meeting was concluded with the annual dinner of SPEE, which was attended by members of the Association.

Three sessions on June 22 and 24 were devoted to the following invited and contributed papers.

Invited Papers

We taught engineering physics in the AST and Navy V-12 Programs—Or did we? J. W. WOODROW, *Iowa State College.*

ASTP experiences. W. W. COLVERT, *Illinois Institute of Technology.*

Physics in the V-12 program. R. L. ANTHONY, *University of Notre Dame.*

Accrediting and supplementing AST courses. I. WALERSTEIN, *Purdue University.*

Discussion of postwar problems brought into focus by the AST and V-12 programs in physics. PHYSICS STAFF, *University of Cincinnati.*

Physics instruction for Army Specialized Training Reserves. D. F. MINER, *Army Specialized Training Division.*

Rearrangement of topics in elementary physics courses. T. H. OSGOOD, *Michigan State College.*

The physics war front in Washington. H. L. DODGE, *National Research Council.*

Joint Conference with Electrical Engineering Division on Ultra-High Frequencies and Electronics

Ultra-high frequency developments and classroom and laboratory exposition of amplifier theory. R. I. SARBACHER, *Radio Division, Navy Department.*

The coordination of the work of the physics, mathematics and electrical engineering staffs in the formulation of communications and electronics curriculums, including ultra-high frequency technics. E. A. GUILLEMIN, *Massachusetts Institute of Technology.*

Educational requirements for a production engineer. J. A. HUTCHESON, *Westinghouse Research Laboratories.*

Discussion. A. B. BRONWELL, *Northwestern University.*

Dinner Meeting with Electrical Engineering Division.

Anchors aweigh. OVID W. ESHBACH, *Northwestern Technological Institute.*

Some revisions of engineering education. ALAN HAZELTINE, *Consultant.*

Contributed Papers, with Abstracts

1. The use of electrical units in elementary physics. JOSEPH H. HOWEY, *Georgia School of Technology.*—A survey of elementary textbooks using the mks units shows that most authors have been hesitant to switch entirely over to this system in electricity. Apparently the tendency to use different systems of units in different situations will persist for some time. The desirability of being able to choose a unit of convenient size for the purpose at hand is recognized, as is the desirability of eliminating the multiplicity of systems of units. It is hoped that the exclusive adoption of the practical mks system will be hastened when it is better recognized that selected units of convenient size can be used from another system as sub-multiples or multiples of the corresponding mks units; the whole system need not be introduced, nor need its existence even be mentioned. In using mixed units in this way it is only necessary to let the symbols in equations represent both a number and a name of a unit with the understanding that the names of the units are to be carried through all algebraic processes and not discarded except by cancellation. This procedure, often used in mechanics, has never been very feasible in electricity with the gaussian system of units, but it is quite usable with the mks system; it is further recommended because it implicitly offers the advantage of dimensional analysis without introducing additional terminology or notation.

2. How not to teach the meaning of mass. W. H. MICHENER, *Carnegie Institute of Technology.*—This is a plea for simplicity and clarity in the treatment of mass, or inertia, in textbooks. Confusion arises from (*a*) referring to mass as a quantity of matter, (*b*) calling a body a mass, (*c*) defining mass as a property of a body by virtue of which it has another property, (*d*) overemphasizing the invariability of mass before mass is defined, (*e*) stating or implying that mass, being a fundamental quantity, is indefinable. The word *mass* should be used only in the sense of inertia. It should be defined with the aid of Newton's laws of motion. It is only by means of careful experiments

that we learn that the mass of a body is usually invariable, and that the masses of two different bodies are proportional to their weights.

Attendance

Fifty-one members and nonmembers registered at the meeting. Members who registered were:

R. L. Anthony, University of Notre Dame; I. A. Balinkin, University of Cincinnati; C. E. Bennett, University of Maine; W. H. Billhartz, Franklin College; W. W. Colvert, Illinois Institute of Technology; H. L. Dodge, National Research Council; J. D. Elder, Wabash College; H. M. Fry, Franklin and Marshall College; J. H. Gibbud, Denison University; J. L. Glathart, Kenyon College; M. B. Grandy, University of Dayton; A. Hazeltine, Stevens Institute of Technology; F. F. Householder, University of Akron; K. Lark-Horovitz, Purdue University; J. H. Howey, Georgia School of Technology; W. H. Michener, Carnegie Institute of Technology; T. H. Osgood, Michigan State College; C. J. Overbeck, Northwestern University; J. G. Potter, Bell Telephone Laboratories; S. O. Grimm, Franklin and Marshall College; D. W. Stebbins, Wright Field; V. C. Stechschulte, Xavier University; C. B. Vance, Evansville College; L. Vollmayer, Xavier University; I. Walerstein, Purdue University; C. Williamson, Carnegie Institute of Technology; M. W. White, The Pennsylvania State College; J. W. Woodrow, Iowa State College.

JAMES G. POTTER,
Program Chairman

Rochester Meeting, June 23 and 24, 1944

The American Association of Physics Teachers met at the University of Rochester on June 23 and 24, 1944, concurrently with the American Physical Society. PROFESSOR LOUISE MCDOWELL, Vice President of the Association, was in charge of the program. Professors McDowell and R. C. Gibbs presided.

The dinner of the two societies, held on June 23 at the Hotel Seneca, was followed by an address by DR. C. E. K. MEES on "The new world and the scientist." Another feature of the meeting was the inaugural program of the new Division of High Polymer Physics of the American Physical Society.

Of particular interest were the various demonstrations given in connection with several of the contributed papers, particularly the demonstration on a large scale of the value of color, size and illumination in making visible ordinary lecture experiments and equipment. The symposium on Essential Physics for a Naval Career clearly illustrated the advantages in teaching physics to a group of students to whom the applications of essential principles are of immediate importance; it was especially helpful to those who are teaching in the Navy V-12 program.

At the close of the symposium on The Training of Physicists for Industry, there was an animated discussion, opened by W. B. RAYTON and HARVEY FLETCHER. In both the papers and this discussion it was clearly brought out that the greatest need for improvement lies in training at the undergraduate level where greater emphasis should be placed on a thorough understanding of fundamental principles and the ability to apply them to new problems. The chief deficiencies in the training of the graduate going directly into industry were felt to be: (i) the ability to get things done, "engineering know-how"; (ii) the attitude of mind that leads to good team work; (iii) the ability to write clear concise reports. Despite the emphasis on engineering and military physics, in the various sessions, it was continually emphasized that liberal arts students be given a clear understanding of the role of physics and of what the scientific method really is.

Four sessions were devoted to the following invited and contributed papers.

Invited Papers

Rearrangement of topics in elementary physics courses. T. H. OSGOOD, *Michigan State College.*

Elements entering into a norm by which physics department might rate themselves. WHEELER P. DAVEY, *The Pennsylvania State College.*

The Navy V-12 program at Rochester. HERBERT R. CHILDS, *University of Rochester.*

The V-12 program: proving ground for postwar training in physics. G. E. GRANTHAM, *Cornell University.*

Physical science courses for liberal arts students. REGINALD J. STEPHENSON, *University of Chicago.*

Symposium: Essential physics for a naval career. F. K. ELDER, J. A. TIEDEMAN, L. E. KINSLER, J. D. RIGGIN, E. R. PINKSTON, R. A. GOODWIN, *United States Naval Academy.*

Training of Physicists for Industry

Postwar training of physicists for industry. SAUL DUSHMAN, *General Electric Company.*

Training for the small industrial research laboratory. H. L. MASON, *Taylor Instrument Company.*

Influence of war research on the postwar physics curriculum. BRIAN O'BRIEN, *University of Rochester.*

Observations on the training of physicists. M. N. STATES, *Central Scientific Company.*

Contributed Papers, with Abstracts

1. There are no formulas in physics. ROBERT S. SHAW, *College of the City of New York.*—A common complaint of students is that the study of physics requires the rote learning of a tremendously large number of "formulas." The author believes that this complaint is entirely unjustified, but finds it exceedingly difficult to convince students of the fact. Textbooks are usually written in such a manner as to give a superficial confirmation of the student viewpoint. It is suggested that many topics in physics can be taught completely by the use of nonalgebraic statements of principles, coupled with a more generous allotment of solved problems, and that such a treatment would be preferable when possible. Certain topics are already so treated—for example, Archimedes' principle—but the possibilities are by no means exhausted. Some errors are actually induced by the use of an algebraic symbolism, which can conceal truth as well as reveal it. There is a serious question concerning the ethics of using a result that "can be derived using methods beyond the scope of this book." Perhaps a much-needed reduction in the amount of material to be included in a first course could

be secured by selecting only those topics for which the title of this paper can be made true. There is no intention to substitute a qualitative for a quantitative treatment; physics is essentially a quantitative subject, but is *not* a rote-memory subject.

2. Principles of physics applied to traffic movements and road conditions. ERNA M. J. HERREY, *Queens College, New York.*—No comprehensive attempts have been made so far to apply methods of physics systematically to all relations between traffic movements and road conditions. It is suggested that a productive approach is possible, if traffic is not considered as an accumulation of many independently moving vehicles, but as a continuous stream of changing density, which consists of the influence spaces of the vehicles. These are the spaces which the vehicles require around themselves in order to proceed without interference with other objects. The sizes and movements of these spaces in relation to one another and to the road are functions of physical properties of road and of traffic itself—speed, friction between road surface and tires, vehicle sizes, and so forth—and are expressed in sets of equations that interpret characteristic phases of this continuous stream. Such a phase is effectively illustrated, for instance, in the case of the procedure of the stream at a typical road intersection. A convenient kind of graphical representation in time-way charts shows procedure and mutual relations of the influence spaces. This approach is of practical value in war conditions and for road planning in the postwar time. It lends itself also well to classroom discussion and as a demonstration of how methods of physical research are applied to practical problems that are familiar to students in every detail and accessible without much advanced training.

3. A demonstration water hammer made of metal. HAROLD K. SCHILLING and HENRY L. YEAGLEY, *The Pennsylvania State College.* The complete note appears under "Notes and Discussion" in this issue.

4. Illumination and color in demonstration experiments. H. K. SCHILLING and MARSH W. WHITE, *The Pennsylvania State College.*—It was the thesis of this paper that demonstration experiments should be made visible to the class. This has not usually been the case because of (i) poor illumination of the apparatus and lecture table, (ii) small-scale apparatus, (iii) apparatus available from instrument companies usually having a black coating—the worst possible color for visibility, particularly when placed on a black-top table, (iv) lack of color or other means whereby significant parts of apparatus can be distinguished by students. Methods were described for minimizing these difficulties and for making demonstration experiments more visible and interesting to students. Satisfactory general illumination on the lecture table is the first requisite for visibility. Modern spot and flood lamps are satisfactory and inexpensive illumination aids. Many small and otherwise nearly invisible pieces of apparatus can be made surprisingly distinct by placing them in front of an illuminated background. A simple but effective background is a white board illuminated by "goose-necked" lamps; a better background is a sheet of opal glass illuminated from behind. Demonstrations were given of some standard pieces of equipment whose parts have been painted different colors to assist in visibly differentiating the essential portions of the apparatus, and also of some specially designed large-scale devices to illustrate this principle.

5. Definitions in mechanics. J. G. WINANS, *University of Wisconsin.*—Definitions of the physical quantities of mechanics may be expressed as an equation $A = B$, with B either defined by some other equation or recognized as undefined. Length, time and force may be considered as undefined. Length and time serve to define area, volume, velocity and acceleration. Mass may be defined as *force/acceleration*. In a similar way angle, time and torque can serve as the basis for definitions of angular quantities. The definition of rotational inertia as *torque/angular acceleration* leads to practically complete correspondence between linear and angular equations of motion. By these definitions and use of a consistent system of units the confusion between mass and weight is eliminated. The relation of mass m and weight W is always given by $m = W/g$. Units may be based on the centimeter, second and dyne, with the dyne defined as the force needed to give 1 cm^3 of water at 4°C an acceleration of 1 cm/sec^2. The gram is then defined as the mass of an object that is given an acceleration of 1 cm/sec^2 by a force of 1 dyne.

6. Elementary demonstration experiments. ERIC M. ROGERS, *Princeton University.*—(1) *Surface-tension changes.* By screen projection it may be shown that alcohol applied to the end of a funnel from which water is dripping makes the drops much smaller—"the unreliable medicine-dropper"—and that wetting-liquid applied to a pool of water on paraffin wax makes it collapse. (2) *To show that three forces in equilibrium must be concurrent,* an irregular board supported on marbles on a table is pulled by three horizontal strings; white threads laid along the lines of the strings meet in a point. (3) *Hydraulic analog of Ohm's law.* Water flows through a coiled "resistance" tube. The pressure difference between the ends is indicated by a mercury manometer, while a simple flow-meter in series with the tube indicates the current.

Attendance

Seventy members and guests registered at the meeting. Members in attendance were:

Mildred Allen, Mount Holyoke College; Gladys Anslow, Smith College; E. F. Barker, University of Michigan; Hilda Bass, University of Rochester; H. A. Barton, American Institute of Physics; N. F. Beardsley, University of Chicago; L. Bockstahler, Northwestern University; E. C. Campbell, Princeton University; G. H. Carragan, Rensselaer Polytechnic Institute; K. K. Darrow, Bell Telephone Laboratories; W. P. Davey, The Pennsylvania State College; D. C. Duncan, The Pennsylvania State College; J. A. Duncan, Naval Ordnance Test Station (Calif.); F. K. Elder, U. S. Naval Academy; F. C. Fairbanks, University of Rochester; W. R. Fredrickson, Syracuse University; R. C. Gibbs, Cornell University; A. C. Haussman, Hobart College; Erna M. J. Herrey, Queens College; A. King, Dartmouth College; R. F. Kingsbury, Bates College; L. E. Kinsler, U. S. Naval Academy; G. A. Lindsay, University of Michigan; W. N. Lowry, Bucknell University; K. V. Manning, The Pennsylvania State College; Louise McDowell, Wellesley College; W. C. McQuarrie, Lafayette College; T. H. Osgood,

Michigan State College; R. A. Porter, Syracuse University; E. M. Rogers, Princeton University; D. Roller, Wabash College; F. L. Roth, National Bureau of Standards; G. K. Schoepfle, Oberlin College; R. S. Shaw, College of the City of New York; M. N. States, Central Scientific Company; R. J. Stephenson, University of Chicago; J. A. Tiedeman, U. S. Naval Academy; F. L. Verwiebe, Hamilton College; B. B. Watson, University of Pennsylvania; R. L. Weber, The Pennsylvania State College; F. A. White, University of Rochester; M. W. White, The Pennsylvania State College; W. R. Wright, Swarthmore College.

LOUISE MCDOWELL,
Vice President

Salem Meeting of the Oregon Section

The Oregon section of the Association met at Willamette University, Salem, Oregon, May 13, 1944. At the afternoon session a symposium on laboratory teaching was presented in conjunction with an exhibit of laboratory equipment arranged by Willamette University and the University of Oregon. The exhibit was discussed by Stanley Minshall, *University of Oregon*, and P. N. Spelbrink, *Willamette University*. The following papers were presented:

Laboratory thoughts. J. C. Garman, *Oregon State College.*
Laboratory ideas. H. L. Esterly, *Multnomah College.*
Laboratory objectives. E. H. Collins, *University of Oregon.*
Laboratory experiences. W. R. Varner, *Oregon State College.*

Following a dinner at the Hotel Salem, D. M. Goode, of Oregon State College, spoke on "The teacher's job," analyzing the place of educational institutions in a democratic system of world order and the function of the teacher, particularly the college teacher, in the educational process.

W. B. Anderson, Oregon State College, was elected President of the section for the coming year; E. H. Collins, University of Oregon, was re-elected Secretary.

The next meeting of the Section will be held November 4, 1944, at Willamette University.

E. HOBART COLLINS,
Secretary

Atlanta Meeting of the Southeastern Section

The tenth annual meeting of the Southeastern Section of the American Physical Society was held in the Atlanta Biltmore Hotel and at the Georgia School of Technology, Atlanta, Georgia, on May 5 and 6, 1944. Approximately 65 members and guests attended. Local arrangements were made by a committee headed by J. H. Howey.

The regular program consisted of eight papers, abstracts for three of which appear here. Abstracts of the remaining papers will be found in the June 1 and 15 issue of *The Physical Review*.

1. The equitable adjustment of a set of unsatisfactory grades. MILTON L. BRAUN, *Catawba College.*—An equation has been derived for the conversion of an unsatisfactory class average (or median) grade of b into a standard or acceptable value a, thereby converting the student's attained grade of g into an equitably adjusted grade y. This equation is

$$y=g\frac{100-a}{100-b}+\frac{100(a-b)}{100-b},$$

which, when used with a slide rule, is less formidable than it appears. Usually $a>b$ and $y>g$.

2. On the rationalization of the teaching of electrodynamics. ELLIOTT T. BENEDIKT, *University of Florida.*—The teaching of electrodynamics is at present generally based on the imaginary concept of "magnetic pole" which is introduced in order to define the concept of magnetic field. The historical origin of this concept and its consequences for the development of electrodynamics was discussed, together with the origin of Ampère's definition of the intensity of an electric current. It was shown that a more rational treatment can be obtained by taking as point of departure the expression for the force between two electric charges. If the charges are at rest, this force is expressed by Coulomb's law. If, more generally, the charges are in motion, there is an additional term, which is rather complicated but which can be greatly simplified by the introduction of an auxiliary vector quantity, called "magnetic field." All the formulas relative to electromagnetic induction can be derived from the expression for the force between moving charges. In general, Maxwell's equations can be deduced by assuming as experimental facts Coulomb's law and the principle of restricted relativity.

3. An experiment on teaching lens theory in elementary optics. F. T. ROGERS, JR., *University of Houston.* (By title.)—A project is under way in connection with a course in elementary optics, which, it is hoped, may result in impressing students with (i) the practical limitations of the first-order lens theory, and (ii) the intricacies of lens-design procedures. The basis of the experiment is the design of a fairly good, small, telescope objective lens. The procedure consists first in finding the powers D_1 and D_2 of crown and flint components (with dispersion constants ν_1 and ν_2, respectively) which in contact will produce an achromatized pair of power D, in accord with the equations $D_1/D_2=-\nu_1/\nu_2$ and $D=D_1+D_2$. Then the student must find, in whatever elementary way he can devise, such radii of curvature as will largely eliminate the spherical aberration of the pair in its specified application.

Additional features of the program were a talk by H. T. Briscoe of the Professional and Technical Division, Bureau of Training, War Manpower Commission, on "Manpower problems in the sciences," an address at the annual dinner by H. L. Dodge, of the Office of Scientific Personnel, and a review by F. L. Brown of some of the papers presented at the recent New York meeting of the American Association of Physics Teachers.

At the business meeting the election of the following officers was announced: *Chairman*, O. T. Koppius; *Vice Chairman*, F. L. Brown; *Secretary*, E. Scott Barr; *Treasurer*, C. B. Crawley; *Member of the Executive Committee*, Eric Rodgers.

E. SCOTT BARR,
Secretary

District of Columbia and Environs

The sixth annual meeting of the District of Columbia and Environs section was held on April 22, 1944 at George Washington University. About 40 members and guests attended the morning and afternoon sessions.

The following contributed papers were heard:

Wavelength of progressive sound waves. G. D. ROCK, *Catholic University.*

Power factor measurements with an oscilloscope. T. B. BROWN, *George Washington University.*

Demonstrations of the Doppler effect. F. E. FOX, *Catholic University.*

Forces on rigid bodies. C. A. BECK, *Catholic University.*

Device for teaching thin lenses. R. C. HITCHCOCK, *U. S. Naval Academy.*

The effect of resistance upon decrement. FR. DOOLING, *Catholic University.*

Classroom demonstration of lens aberrations. A. S. JOHNSON, *U. S. Naval Academy.*

Ratio of maximum to effective values in a.c. circuits. FR. FLYNN, *Catholic University.*

The radiometer. R. A. GOODWIN, *U. S. Naval Academy.*

Radio oscillator range extender. W. T. WHELAN, *Catholic University.*

Visual demonstration of shm relations. F. L. TALBOTT, *Catholic University.*

Projection apparatus for gas laws. T. B. BROWN, *George Washington University.*

Other events of the meeting were a noon luncheon at the Faculty Club and the annual business meeting. The following officers were elected for the college year 1944–45: *President*, F. K. Elder; *Secretary*, W. A. Kilgore; *Executive committee*, F. L. Talbott, George M. Koehl and John A. Tiedeman.

W. A. KILGORE,
Secretary

Curriculum Changes at Massachusetts Institute of Technology

CHANGES in the undergraduate curriculum of the Massachusetts Institute of Technology to meet the requirements of postwar education have just been made as a result of months of study by a special faculty committee. The future of technical education in relation to significant wartime advances in science was taken into consideration by the committee. However, the most important feature of the revision is a coordinated series of courses in the humanities and social sciences extending throughout the four years.

The new schedule provides a standard curriculum for all first-year students. The second-year program has two main subdivisions, *science* and *engineering*, with an opportunity for the student to begin elementary subjects of his chosen profession. The number of undergraduate subjects was reduced from 523 to 412 by consolidating courses of nearly the same content and purpose, and eliminating certain options. There was also a reduction in the variety of schedules offered in the upper years.

The four-year program provides that, in addition to customary courses in modern languages, students will take one full course of a nonprofessional character every year. In the first year this work will be devoted to English, with emphasis on written and oral expression. In the second year it will be an introductory course in modern history with special reference to the place of the United States in world affairs. On the basis of the preceding courses, especially the history, the third year will involve the social sciences generally, economics and psychology; in the last half of this year students may choose among a limited number of options in the social sciences.

Senior students can choose one of four options—history of science and thought, music and the fine arts, western world literature, or international relations. Each of these courses will be closely coordinated in objectives and purposes with those given in the earlier years. All class sections will be small, and continual opportunity will be given for improvement in both written and oral expression.

In its social outlook this four-year plan is an expansion of courses that have long been a part of the Institute's curriculum. Even as early as 1865 when the Institute opened its doors, President William Barton Rogers was a pioneer in his conviction that a sound scientific or technical education could not be complete without adequate time and attention to the humanities and the social sciences.

RECENT PUBLICATIONS AND TEACHING AIDS*

ADVANCED AND INTERMEDIATE TEXTBOOKS

Quantum chemistry. HENRY EYRING and JOHN WALTER, Princeton University, and GEORGE E. KIMBALL, Columbia University. 400 p., 21×14 cm. *Wiley*, $5. Planned as a graduate course for students of chemistry and thus used in substance at Princeton University since 1931, this textbook covers a wider range of subject matter than is ordinarily found in introductory books on quantum mechanics. Chemistry, because of its complexity, must remain in large measure an experimental science. Nevertheless, as the present authors say, few chemists of today can afford to be uninformed about a theory that systematizes all of chemistry, even though mathematical complexities often put exact numerical results beyond his immediate reach.

Micromeritics, the technology of fine particles. J. M. DALLAVALLE. 442 p., 100 fig., 82 tables, 15.5×23.5 cm. *Pitman*, $8.50. *Micromeritics*, a new term, is adopted by the author of this textbook to denote the science and technology of particles ranging in size from about 10^{-1} to $10^{5}\mu$. The scope of the science and its relation to fields such as soil and mineral physics and hydrology can be seen from the following partial list of main topics treated: dynamics of particles; shape and size distribution; theory of sieving and grading of materials; characteristics of packing; electrical, optical, sonic and chemical properties; thermodynamics of particles; flow of fluids through packings; capillarity; mud and slurries; theory of fine grinding; atmospheric and industrial dust. A knowledge of elementary physics and calculus is assumed. There are lists of problems and an extensive bibliography.

Structure of metals. CHARLES S. BARRETT, Associate Professor of Metallurgical Engineering, Carnegie Institute of Technology. 580 p., 15.5×23 cm. *McGraw-Hill*, $6. This book is intended to serve both as a reference work and as a textbook in courses for senior and graduate students of metallurgy. The first seven chapters cover the fundamentals of crystal lattices and projections, and the principles and technics of x-ray diffraction. The last half of the book reviews researches of current interest, including such problems as the principles governing crystal structure, superlattices, plastic deformation, effects of cold work and annealing, x-ray studies of internal stresses, and the structure of liquid metals. Many literature references are given.

Vector and tensor analysis. HOMER V. CRAIG, Professor of Applied Mathematics and Astronomy, University of Texas. 448 p., 15.5×23.5 cm. *McGraw-Hill*, $3.50. Being intended for students who will use vector and tensor analysis as a tool and who should therefore be introduced to the subject as early in their program as possible, this textbook does not presuppose much initial knowledge of advanced calculus. The knowledge that will be needed can be obtained from the first five chapters—on mathematical background; indeed, as the author states, there is some advantage in thus incorporating part of advanced calculus into a subject of physical nature where it is needed. Chapters 6 to 9 deal with elementary vector analysis; chapters 10 and 11, with tensors and extensors; and the final three chapters, with applications to classical dynamics, restricted relativity and general relativity. The main emphases in the book are on applications of vector analysis to topics in advanced calculus, on the use of tensor notation and on invariance of functional form.

The mathematics of physics and chemistry. HENRY MARGENAU, Associate Professor of Physics, and GEORGE MOSELY MURPHY, Assistant Professor of Chemistry, Yale University. 593 p., numerous figs. and tables, 16×23 cm. *Van Nostrand*, $6.50. A book of senior and first-year graduate grade that presents those parts of mathematics which form the tools of the modern theoretical physicist and chemist. Each main topic is logically developed, with proofs given where omission would destroy the continuity of treatment. The degree of rigor employed, as the authors point out, is that customary in careful scientific demonstrations—both sloppiness and the opposite extreme of rigor mortis are avoided; for, "if the history of the exact sciences teaches anything it is that emphasis on extreme rigor often engenders sterility, and that the successful pioneer depends more on brilliant hunches than on the results of existence theorems." There are 14 chapters: Mathematics of thermodynamics, Ordinary differential equations, Special functions, Vector analysis, Vectors and curvilinear coordinates, Calculus of variations, Partial differential equations, Eigenvalues and eigenfunctions, Mechanics of molecules, Matrices and matrix algebra, Quantum mechanics, Statistical mechanics, Numerical calculations, Linear integral equations, Group theory. The book should be highly useful as a reference work or as a textbook for a course in applied mathematics. Selected parts could be readily used in introductory or review courses on special subjects. Problems and worked examples are provided, and there are numerous literature references.

FIRST-YEAR TEXTBOOKS

Physics. Ed. 4. O. M. STEWART, Late Professor of Physics, University of Missouri. 795 p., 500 fig., 15×23 cm. *Ginn*, $4. The last edition of a highly successful general textbook, seen through the press by its author only shortly before his death. PROFESSOR STEWART took into account in the revision many suggestions sent to him by teachers using the book. He revised the chapters on vibratory motion and on meteorology, and augmented the material on electronics, high frequency oscillations and some other modern topics. However, as in the previous editions, fundamental aspects rather than technical uses have been

* The addresses of the publishers of these books are: Blakiston Co., 1012 Walnut St., Philadelphia 5, Pa.; Jaques Cattell Press, N. Queen St. and McGovern Ave., Lancaster, Pa.; Ginn and Co., 15 Ashburton Place, Boston 17, Mass.; Macmillan Co., 60 Fifth Avenue, New York 11, N. Y.; McGraw-Hill Book Co., 330 West 42 St., New York 18, N. Y.; Pitman Publishing Co., 2 W. 45 St., New York 19, N. Y.; D. Van Nostrand Co., 250 Fourth Ave., New York 3, N. Y.; John Wiley & Sons, 440 Fourth Ave., New York 16, N. Y.; Garden City Publishing Co., Garden City, N. Y.

stressed. Both cgs and mks units are used, but the fps absolute system is omitted. The many problems are new.

SURVEY COURSE TEXTBOOKS

Man's physical universe. Rev. ed. ARTHUR TALBOT BAWDEN, President of the Stockton Junior College. 847 p., 320 fig., 16×24 cm. *Macmillan*, $4.

Man and his physical world. DWIGHT E. GRAY, Associate Professor of Physics, The University of Akron. 677 p., 218 fig., 16×23 cm. *Van Nostrand*, $3.75.

Physical science. WILLIAM F. EHRET (Editor), Professor of Chemistry, LESLIE E. SPOCK, JR., Associate Professor of Geology, WALTER A. SCHNEIDER, Associate Professor of Physics, CAREL W. VAN DER MERWE, Professor of Physics and HOWARD E. WAHLERT, Instructor in Mathematics, Washington Square College, New York University. 649 p., 16×24 cm. *Macmillan*, $3.90.

The authors of these textbooks for survey courses assert similar aims for such courses. In PRESIDENT BAWDEN's words, the purpose of a survey course is "to try to fit together the thin individual slices of knowledge that science has prepared and to discover a pattern running through all of the sciences that will provide not only intelligible but useful knowledge." All the authors assert that it is their aim "to give the reader some appreciation of science and its methods. . . . To appreciate the role of science in *our* civilization, we must understand the basic principles of pure science." They all seek to achieve these aims by teaching facts and the methods of discovering them in astronomy, geology, chemistry and physics. In none of the three books here reviewed is there complete separation of these four fields, although in all the overlapping is most marked in chemistry and physics.

The first edition of PRESIDENT BAWDEN's book appeared in 1937 and was reviewed in this journal [Am. J. Phys. **6**, 74 (1938)]. As before, the social implications of the development of physical science are kept constantly in mind. A large amount of information is presented, much of it dealing with modern developments in pure science and in technology. No mathematics at all is used, and quantitative results are given infrequently. Although a number of errors or confused statements noted by the previous reviewer have been corrected, one still finds statements that will at least need clarification by the instructor.

PROFESSOR GRAY's book is similar in purpose and content to PRESIDENT BAWDEN's; it "endeavors to maintain a close liaison between its subject matter and everyday experience and knowledge" by frequent illustrations from familiar fields. Perhaps because of its smaller compass, fewer facts are presented, but they are well chosen and fitted together. Only the simplest algebra is used; a chapter on number systems contains interesting but perhaps irrelevant material on Roman, Greek, Mayan and Hindu-Arabic notations.

It may be that the collaboration of specialists in four fields tends to produce a book having much more the appearance of a standard textbook than has either of the preceding. PROFESSOR EHRET and his collaborators do not hesitate to use simple mathematics, including the trigonometric functions, but no mathematical skill is assumed on the part of the student. Fundamental equations, such as those of motion, of the force between charges, and of lenses, are presented in the usual way and are used in solving problems.

All three books contain extensive bibliographies or lists for supplemental reading. All have questions or exercises for further study; PROFESSOR GRAY gives at the end of each chapter three sets of questions—discussion, multiple-choice and true-false.

ASTRONOMY AND NAVIGATION

Galaxies. HARLOW SHAPLEY. 236 p., 126 fig., 15×22 cm. *Blakiston*, $2.50. Another volume in the excellent series of nine comprising *The Harvard Books on Astronomy* [Am. J. Phys. **10**, 271 (1942); **11**, 166 (1943)]. We should like to reiterate that in physics, too, the need is great for reference books of this sort—monographs written by specialists in their respective fields, yet geared to the interests and capacities of serious-minded beginners.

Celestial navigation—A problem manual. WALTER HADEL, First Navigator and Chief Navigation Instructor, United Air Lines. 274 p., 51 fig., 4 tables, 14×21.5 cm. *McGraw-Hill*, $2.50. A textbook consisting of a collection of practical, modern problems in celestial navigation, with each type of problem fully explained by a worked example. Various calculations in simple dead-reckoning navigation are included. The book is the outgrowth of a course given by the author in 1943 at a United Air Lines training center.

Marine and air navigation. JOHN Q. STEWART and NEWTON L. PIERCE, Princeton University. 484 p., many illustrations, 8 color plates, 47 tables, 22.5×28.5 cm. *Ginn*, $4.50. A comprehensive and modern textbook for beginners in navigation—unique in that it provides an integrated treatment of both marine and air navigation, a mode of approach believed by the authors to be most valuable even for those students who must follow an accelerated or condensed program. The treatments of navigational charts and of map projection in general are unusually thorough. The authors recognize that the practicing navigator can do with a minimum of astronomy, but for those who want it they have supplied a full discussion of nautical astronomy in a separate chapter. Many worked examples and 275 problems are provided. The illustrations, charts and book work are good.

ELECTRICAL ENGINEERING

Electrical engineering: basic analysis. EVERETT M. STRONG, Professor of Electrical Engineering, Cornell University. 403 p., 14.5×22 cm. *Wiley*, $4. The author has sought to lead the beginner in electrical engineering into the field "with as much zest and interest as possible, yet also with more than the usual attention to accuracy of statement and unambiguity of meaning." In these several respects he appears to have succeeded admirably. The style is lively, yet the treatment is analytical rather than merely descriptive, is quite critical in its approaches to topics and exhibits more than ordinary regard for the selection and use of good terminology. The prerequisites are one year of physics and elementary calculus. Alternating as well as direct currents are treated, but without presenting the latter as a special case of the former, a

procedure that some teachers of electrical engineering now appear to be advocating. Many problems and interesting questions are provided. In the back of the book there are detachable work sheets containing various graphs, which are intended for collection and re-issue by the instructor in closed-book examinations. With this device, a book is also more likely to end up in the student's permanent library.

METEOROLOGY

Ways of the weather. W. J. HUMPHREYS, U. S. Weather Bureau. 400 p., 75 fig., 16×24 cm. *Jaques Cattell*, $4. Written in a delightful popular style, this volume gives a quite comprehensive account of weather and related phenomena that should prove to be attractive to the general reader and to the student using it as collateral reading in elementary courses.

Meteorology, theoretical and applied. E. WENDELL HEWSON and RICHMOND W. LONGLEY, Meteorological Service of Canada. 280 p., 194 fig., 14×22 cm. *Wiley*, $4.75. Designed to serve both as a textbook for a substantial introductory course of college grade and as a modern reference work for practicing forecasters, this book seeks to give a more than ordinary integration of forecasting technic with the theory on which it is founded. A knowledge of elementary physics and calculus is assumed. About one-fifth of the book deals with basic theory; the remainder, with applications. These two parts may be studied simultaneously, if desired. A novel feature of the theoretical part is the chapter on statistical analysis of meteorological data.

MISCELLANEOUS BOOKS

Steel in action. CHARLES M. PARKER, American Iron and Steel Institute. 226 p., 18 fig., 14×20 cm. *Jaques Cattell*, $2.50. A popular but authoritative description of the raw materials, technology and economics of steel manufacture. This book is one of the *Science for War and Peace Series*.

Atoms in action. GEORGE RUSSELL HARRISON, Professor of Physics, Massachusetts Institute of Technology. 413 p., 15×22 cm. *Garden City Publishing Co.*, $1.49. PROFESSOR HARRISON's book is now available for the first time at a popular price, and every physicist should help in bringing this fact to the attention of the reading public and all librarians.

Basic mathematics for war and industry. PAUL H. DAUS, Professor of Mathematics, University of California, Los Angeles, JOHN M. GLEASON, Assistant Professor of Mathematics, San Diego State College, WILLIAM M. WHYBURN, Professor of Mathematics and Educational Supervisor, ESMWT, University of California, Los Angeles. 288 p., 277 fig., 14.5×22 cm. *Macmillan*, $2. A textbook for a course covering those elementary principles of arithmetic, algebra, plane and solid geometry, and plane and spherical trigonometry that are most needed in various practical situations of present-day interest. It should be useful for courses of the ESMWT and vocational types and also as a reference work in connection with the more elementary physics courses. Because of the latter possibility, one wishes that the authors had been a little less sloppy in expressing the numerous physical quantities employed in the worked examples and problems, particularly since the purely mathematical material has been organized and treated with care and respect.

PAMPHLETS

Methods of using standard frequency broadcasts by radio. Letter Circular 751. *National Bureau of Standards* (Washington 25, D. C.), gratis.

The romance of electricity. GED-1073. 55 p. *General Electric Co.* (Publicity Div., Schenectady, N. Y.), gratis. A popular account of electrical progress, attractively written and printed.

Teaching aids. 17 p. *Westinghouse Electric & Manufacturing Co.* (School Service, 306 Fourth Ave., Pittsburgh 30, Pa.), gratis. Available booklets, charts, motion pictures, slide films and other free or low cost services to schools are described.

Du Pont Publications. *E. I. du Pont de Nemours & Co.* (Electroplating Div., Wilmington 98, Del.), gratis. The following pamphlets are of interest to physicists: Electroplating chemicals, processes, materials; Electrodeposition of gold and silver; High speed copper electroplating process; Electrotinning; Bright zinc "Durobrite"; Heavy silver plating; Hull and Strausser tests for zinc, cadmium, tin and copper; "Moly-black."

How can I avoid wasting light? *Westinghouse Lamp Division* (Advertising Dept., Bloomfield, N. J.), 1 ct. For housewives.

Automobile user's guide. 64 p. *General Motors* (Customer Research staff, Detroit, Mich.), gratis. A new edition of a valuable pamphlet.

CHARTS

Westinghouse charts. *Westinghouse Electric & Manufacturing Co.* (School Service, 306 Fourth Ave., Pittsburgh 30, Pa.), gratis. (1) *Everyday electricity*, a series of charts, 25×38 in., on familiar electrical devices; (2) *Pictorial bulletins*, photographic posters, 14×17 in., on scientific and industrial themes.

D.c. magnetization curves for various magnetic materials. GEL-837. 51×63 cm. *General Electric Co.* (Educational Sales Sec., Schenectady, N. Y.), gratis to colleges. Smaller prints, 20×27 cm, are available for individual use.

Storage battery. *Electric Storage Battery Co.* (Philadelphia, Pa.), gratis.

Compound microscope. *Spencer Lens Co.* (Buffalo, N. Y.), gratis. Smaller prints, 8½×11 in., are also available for individual use.

SLIDEFILMS

Air-age physics; mechanics. *Jam Handy Organization* (29 E. Grand Blvd., Detroit 2, Mich.), for sale. A series of 15 discussional-type slidefilms, totalling 638 individual annotated photographs, charts and drawings, designed to establish a visual and mathematical relationship between basic principles and their practical application. The 15 subjects, each of which is complete in itself, are: Matter, Units of measurement, Force, Force and velocity as vectors, Uniform motion, Uniformly accelerated motion, Newton's laws of motion, Gravitation, Rotary motion, Centrifugal force, Work, Energy, Power, Friction, Simple machines.

DIGEST OF PERIODICAL LITERATURE

Axipetal Force and Acceleration

On a horizontal disk is mounted a thin vertical board, displaced sufficiently from a diameter of the disk that a pendulum supported from the top of the board hangs in the axis of the disk. The disk can be made to rotate by a spring motor at speeds between 60 and 110 rev/min; the speed is kept constant at any desired value by a regulator on the motor. The pendulum bob has a mass of about 7 gm and the pendulum length can be varied between 7 and 13 cm. The board is marked with a centimeter scale for measuring the length of the pendulum and with a set of lines 5° apart radiating from the point of support of the pendulum for measuring the deflection of the latter.

In use, the apparatus constitutes a conical pendulum of length l and mass m, making an angle θ with the vertical and rotating with angular velocity ω. When the velocity ω, as the disk is set into rotation, reaches the value $(g/l)^{\frac{1}{2}}$, the pendulum begins to deflect; the deflection becomes constant when ω attains the value determined by the regulator on the motor. The bob of the pendulum then moves in a circle of radius r with an acceleration $\omega^2 r$, or $\omega^2 l \sin\theta$, and the condition for equilibrium is $mg \tan\theta = m\omega^2 l \sin\theta$. This equation is verified experimentally; ω is determined by timing a number of revolutions of the turntable, and l and θ are measured on the vertical board.

For advanced classes, a study may be made of transient motions and oscillations of the pendulum; the oscillations can be observed with the apparatus but they soon die out.—P. L. TEA, *J. Frank. Inst.* **237**, 131–137 (1944).

Simple Apparatus for Study of the Gas Laws

The laws of Boyle and of Charles can be approximately verified with very simple apparatus. A 250-ml Erlenmeyer flask is provided with a two-hole rubber stopper; in one hole is inserted the tip of a 50-ml burette, and through the other passes a 60- to 80-cm length of 2- or 3-mm capillary tubing, which serves as a manometer. The tube is graduated every 0.2 cm for the lowest 2 cm and every centimeter thereafter; the markings are made with crucible marking ink. The volume of the flask is found with the aid of a graduated cylinder. Then 50 ml of dibutyl phthalate is poured in and the level read on the manometer tube, the burette being empty and its stopcock open; the barometric reading is also obtained. Temperature equilibrium can be obtained by immersing the flask in water in a large beaker. The burette is now filled with dibutyl phthalate, which is added to the liquid in the flask in sufficient increments to produce pressure increments of 5 cm, the corresponding volume changes being obtained from the burette readings. The products of the manometer readings and the volumes are not quite constant, but have slightly lower values as the pressure rises; this is presumably due to increased solubility of air in the manometer liquid under increased pressure. This drift is not observed when mercury is used. Light motor oil may also be substituted for the dibutyl phthalate.

The same apparatus may be used for the study of Charles' law. The flask is immersed in a water bath, the temperature of which is slowly raised. The burette stopcock is kept closed. Sufficient time must be allowed for temperature equilibrium to be attained. The accuracy is about 10 percent.

Dalton's law of partial pressures may be verified with similar simple equipment. The side tubes of two suction flasks—one of 250 and the other of 500 ml—are connected by way of a stopcock and short sections of rubber tubing. Each flask is closed with a two-hole rubber stopper through which pass a graduated capillary manometer tube and one arm of a stopcock. Dibutyl phthalate or light motor oil is used as the manometer liquid. With the central stopcock closed, the large flask is flushed out with dry air pumped through the manometer tube with a hand pump and the smaller flask is similarly filled with oxygen from a cylinder or with carbon dioxide from a large Kipp generator stoppered at the top. The stopcocks are closed and the gases are allowed to come to room temperature; the pressures are then equalized by momentarily opening the stopcocks, and the zero readings of the manometers are recorded. Air and oxygen are now forced into the respective flasks until the desired pressures are attained. When the central stopcock between the flasks is opened the gases mix and the average of the two manometer readings gives the pressure. The results are very satisfactory.

A single suction flask with a two-hole stopper can be used for verifying Graham's law of effusion and for comparing the relative viscosities of gases. Through one hole of the stopper passes a pipette; through the other passes one arm of a stopcock. An orifice is made by piercing a piece of aluminum foil with a sharp needle; the foil is cemented or waxed over the end of a short piece of glass tubing, which is then attached to the open arm of the stopcock with rubber tubing. A second stopcock is attached to the suction tube of the flask. After 50 to 75 ml of liquid has been introduced into the flask, dry air is forced in through the side tube until the liquid has risen to a mark on the upper stem of the pipette. The time required for the liquid level to fall to another mark on the lower stem, while the air escapes from the orifice, is recorded. The molecular weight of another gas, say carbon dioxide, can then be obtained, since the ratio of the molecular weights is equal to the ratio of the squares of the times of efflux.

The glass tube carrying the orifice may be replaced with short lengths of thermometer tubing to determine the relative viscosities of gases. The ratio of the viscosities is equal to the ratio of the times of efflux. This time can be shown to be proportional to the length of the capillary tube. The effect of temperature on viscosity can also be demonstrated.—O. F. STEINBACH AND G. F. CONERY, *J. Chem. Ed.* **21**, 216–218 (1944).

A Criticism of Physics in Engineering Education

We should tie our technology to general principles that are basic. A whole book on strength of materials may expand a few general principles and a few definitions; then may be added a swarm of details. If the student is given the details without the over-all picture he is lost in a maze of things he cannot understand.

Probably the greatest offenders in this respect are found in departments of physics. We have examined a number of textbooks where topics are listed by the hundreds. As a matter of fact we know of a book that has 1500 different items presented to the student. No student can be expected to remember such infinite details.

Probably the greatest criticism of our instruction in the scientific-technological stem is the presentation of great masses of detail. From the very nature of the work some detail is necessary, but we should never lose sight of the principles we are trying to present.—N. W. DOUGHERTY, "Implementing the report of the Committee on Engineering Education after the War," *J. Eng. Ed.* **34**, 641 (1944).

The Isolation of Mathematics

Most mathematicians work with ideas which, by common consent, belong definitely to mathematics. Only a few seek mathematical sustenance in problems arising directly out of other fields of science. This minority does not wish to be labelled "physicist" or "engineer," for it is following a mathematical tradition from Euclid and Archimedes to Newton, Lagrange, Hamilton, Gauss, Poincaré. Neither does the minority wish to belittle in any way the work of the majority, but it does fear that a mathematics which feeds solely on itself will in time exhaust its interest.

The isolation of mathematicians has robbed the rest of science of a support on which it counted in all previous epochs. The technical scientist of today knows a great deal more mathematics than his grandfather knew, but he would be the first to admit that those to whom mathematics is merely a tool cannot take over the role played in the past by the greatest mathematicians of their day. He knows that the mathematician is not merely a skilled user of certain tools already made—he can use those tools with no inconsiderable skill himself. Rather he looks to the qualities peculiar to the mathematician—his logical penetration and capacity to see the general in the particular and the particular in the general.

The fact is that the brain of science is no longer where it used to be. It is not dead or out of order; it has simply been removed by a painless operation and set up apart from the body it so long directed.

It is not the part of a truth-loving mathematician to pretend that change and death occur in the world of ideas any less than in human affairs. If mathematicians have lost their old universal touch—if, in fact, they see the hand of God more truly in the refinement of precise logic than in the motion of the stars—then any attempt to lure them back to their old haunts would be both useless and a denial of the right of the individual to intellectual freedom. If the mathematician of the future is to use his finest efforts within the circle of purely mathematical ideas, let us admit this as a premise and make plans for the future relationship of mathematics to the rest of science. Just now the nonmathematician does not know that the mathematician has given up his general supervision of science, and the mathematician does not realize that he has lost his leadership. What we all need is a genuine and humble attempt to understand the motivation and thought processes of other men of science. If this is achieved there is a hope that, though the mathematician is no longer the great leader, he will remain the friend and counsellor of all science.—J. L. SYNGE, *Am. Math. Monthly* **51**, 185 (1944).

Check List

The new microscopes. R. E. Seidel and M. E. Winter, *J. Frank. Inst.* **237**, 103–130 (1944). A review article of particular interest to students of medicine and biology. A good bibliography is included.

The industrial lot and its sampling implications. L. E. Simon, *J. Frank. Inst.* **237**, 359–370 (1944). Wrong inferences are frequently drawn from samples because of failures to understand what constitutes a lot and to verify the existence of a lot in the practically useful sense. For a discussion of quality control by statistical methods as a field for physicists, see R. H. Bacon, Am. J. Phys. **12**, 157 (1944).

New statistical mechanics. K. K. Darrow, *Bell Sys. Tech. J.* **22**, 362–392 (1943).

Earth-filled walls for x-ray building. Anon., *Eng. News-Rec.* **131**, 636–637 (1943). Thick earth-filled blocks instead of lead are used to shield the operators.

Shielded room design. F. S. McCullough, *Electronic Ind.* **2**, 118 (1943). Constructional details for a radio laboratory room that is shielded with copper sheeting.

Curriculum in engineering physics at the University of Maine. C. E. Bennett, *J. Eng. Ed.* **34**, 556–561 (1944).

Patents and progress. A. Simon, *J. Eng. Ed.* **34**, 398–405 (1944). Those who wish to reform the present patent laws are impatient; for political reasons they are pressing for immediate action on some very radical proposals which, in the opinion of competent unbiased judges, should not be adopted.

Physics in chemical industry. A. J. Mee and R. C. L. Bosworth, *J. Sci. Inst.* **20**, 137–145 (1943). Two articles on some of the important applications of physics in chemical industry and on the best type of training for physicists who expect to enter this field.

Recently published American developments in physical science. C. J. Overbeck, *J. Sci. Inst.* **21**, 1–10 (1944). Some experimental developments during 1942–43.

A simple glass-blowing machine. C. E. S. Phillips, *J. Sci. Inst.* **21**, 17–18 (1944). The contrivance described is not difficult to make and is useful for doing many glass-blowing jobs which normally require more skill and experience than the average person possesses.